THURSDAY, OCTOBER 24, 1889.

JAMES PRESCOTT JOULE.

THROUGHOUT the world of science there has spread a feeling of profound regret at the death of Mr. Joule, which was announced a week ago in the columns of NATURE. On the evening of the 11th of this month he passed away at his residence in Wardle Street, Sale, near Manchester. For many years past he was in very feeble health. Indeed, as long ago as 1872 it was known publicly that he was far from strong. In that year he was President-elect of the British Association, but before the time of the autumn meeting he was obliged to relinquish the honour on account of physical weakness; and Prof. Williamson was called upon to occupy the position. In recent years Mr. Joule was living in complete retirement, carrying on, so far as his health would permit, such observations and experiments as could be conducted without bodily fatigue; and during this period he was able to edit with occasional brief notes, the two volumes of his collected papers which have been published by the Physical Society of London. The first of these important volumes appeared in 1884, and was noticed in NATURE (vol. xxx. p. 27). The second was published in 1887 (see NATURE, vol. xxxv. p. 461), and contained the papers which Joule wrote jointly with Dr. Scoresby, Sir Lyon Playfair, and Sir William Thomson. At the end of the latter volume there is a list of no less than 115 contributions to the various scientific societies and journals which were enriched by communications from his pen. The papers of Joule are remarkable in form as they are in substance. Of mathematics there is scarcely a line; but they are models of clearness, of depth, and of penetration into the hidden things of Nature; and the mathematician finds the experimental results stated and arranged in such a manner as to lend themselves readily to representation in mathematical symbols. Of experimenting he was a perfect master—full of elegant device, and clear in mind as to points of difficulty and places where error might creep in. That which, in the hands of almost anyone else, would have proved too difficult to lead to a trustworthy conclusion, in his hands was often made to yield an important law or generalization, or to afford an accurate numerical result.

In NATURE (vol. xxvi. p. 617) there appeared a biographical sketch of Mr. Joule; a few words may, however, be permitted here, in order to fulfil our duty to one of the greatest scientific leaders of the present century.

His work, taken as a whole, and without considering the relative importance to physical discoveries—that is to say, judged by the originality of the objects, and the means employed, the philosophic direction, the patient and persevering labour, and the results obtained—would be such as to place him in the front rank of philosophers. If account be taken of the importance and generality of his discoveries, as shown by their influence on the philosophical thought and material progress of the world, then, as the discoverer of the law that energy is in the same degree as indestructible and uncreatable as matter, it is with Newton and Dalton that he finds his place in the history of physical science.

The law of the conservation of energy, which in all schools of science, even the most elementary, is now taught as the foundation of each and all the branches of physical science—mechanics, physics, and chemistry—was, in 1841, as far from recognition as at any time since the discoveries of Newton had shown that, in their observed motions, the heavenly bodies strictly obeyed the laws of motion. The properties of friction, internal and external, with which it was found necessary to endow terrestrial material, and which properties were apparently nothing less than those of destroying energy, were still as far from explanation as the property of gravitation itself; while the continued production in the steam-engine of energy from the same material by the agency of heat, without any consumption of heat, as was then not only supposed, but counted as proved by the equality of the heat received from the boiler and discharged into the condenser, showed apparently nothing less than the creation of energy.

The study of heat which had taken place in the meantime had done nothing to remove these difficulties, for the observed fact that heat was susceptible of quantitative measurement, independent of temperature, had led to the hypothesis that heat was an imponderable substance —"caloric"—capable of penetrating matter and altering its temperature and state, but neither creatable nor destructible. The discoveries of electric phenomena tended to strengthen the caloric hypothesis by affording another imponderable substance. Nor was it only such difficulties in the way of the acceptance of the conservation of energy which kept back its discovery.

Energy, as a measure of mechanical potency, had never assumed a prominent place in mechanical philosophy, while the action by which it is converted into motion against resistance, had scarcely been recognized as a general measure of mechanical action, so that when, as continually happened, the idea of heat being a mechanical action thrust itself forward, there was no distinct measure of mechanical action at hand in which to gauge the equivalent.

Outside the schools of mechanical philosophy engineers engaged in constructing and using the steam-engine had long been led to recognize motion against resistance as the mechanical and commercial measure of potency. Under the names "work" and "accumulated work," these men had become familiar with what are now known as work and energy (actual and potential). It may be noticed that Rumford first recognized the true nature of the relation between heat and mechanical action by observing that two horses working steadily produced heat at a steady rate, but he did not reduce his results further; while Joule was so familiar with the action work, that he never hesitated as to the nature of the relation.

It is true that at the time when Joule commenced his work, not only had mechanical philosophy, as applied to astronomy and such abstractions as frictionless and perfectly elastic matter, reached to nearly its highest level, but the other branches of physical science, studied independently, were fast approaching their present stages. Dalton's discoveries of chemical equivalents had been made thirty years before, and chemistry had been advancing by leaps and bounds. The phenomena of electricity had been subject to the masterly handling of

Faraday. The law of the motion of heat had not only been experimentally determined, but had been theoretically discussed by Fourier. The specific heats of the elementary bodies, as well as the heat developed in combination, had to some extent been determined by Dulong. And the law connecting the quantity of electricity produced in voltaic batteries with the number of chemical elements separated, had been discovered by Faraday. So far, however, these various branches of physics and chemistry had been subjects of separate and distinct study. No suggestion of equivalence had been made between the heat of combination of the elements and the electric current that would be produced by the same combination effected in the battery. That heat was developed in conductors by electric currents was known, but again there had been no suggestion of an equivalence between the heat and the resistance overcome. What are now known as the dynamo and motor had been invented by Sturgeon, showing that work could be done by the agency of electricity and electric separation effected by the agency of work, but again no equivalence between the amount of work and the energy of combination of the element in the battery had been surmised.

It was for Joule not only to suggest all these equivalences, but to experimentally determine all their numerical values[1] before he came to the equivalence of the work[2] spent in overcoming fluid, or solid, friction and the heat produced; and, again, between the work spent in compressing air and the heat produced.

The discovery of the mechanical equivalent of heat, important as it is, is but a poor expression for the outcome of this work, in which Joule converted what had till then seemed unquestionable cases of the destruction of energy into the most striking cases of its indestructibility. And although he propounded no theory, but simply declared himself to have believed in the indestructibility of *vis-viva*, or living force, he had, in the truest sense of the word, discovered the universal law that energy is indestructible and uncreatable. As Joule's work came to be apprehended, this law became accepted as its natural consequence in greater and greater significance, until now it stands the most general recognized law in the universe; relating not only to all matter but also the medium of space, which is thus found to possess the mechanical properties of matter and to be subject to the laws of motion.

The discovery of this law, bringing as it does the several branches of physical science into the domain of mechanical philosophy, fitly crowned all the work in physical science of the previous 150 years, of which it was the result. It also opened out a fresh platform for further discoveries—a platform which was immediately occupied in the erection of the compound sciences of thermodynamics, electrodynamics, and the dynamical theory of gases.

Joule was never prominently before the public. Ready to give himself with absolute devotion to the cause of science and the advancement of human knowledge, he yet preferred retirement and the calm labour of his laboratory to the excitement of public lectures and demonstrations. While he was keenly alive to the sympathy of friends, yet he worked for the most part alone, or in conjunction with one or other of the friends mentioned in the earlier part of this notice. He sought not at all for fame, but only for truth.

And yet honours in plenty came to him. He received honorary degrees from the most important Universities; he was honorary Fellow of many learned Societies at home and abroad. He was a Fellow of the Royal Society, and received from it the Gold Royal Medal in 1852, and the Copley Gold Medal in 1870. The Albert Medal of the Society of Arts was delivered to him from the hands of the Prince of Wales in 1880. In 1878 he received a letter from Lord Beaconsfield, announcing that the Queen had been pleased to grant him a pension of £200 per annum. This recognition by his country of his life of scientific labour was a subject of much gratification to Mr. Joule.

Special reference may be made to his connection with the Literary and Philosophical Society of Manchester. This commencing, as it practically did, at the age of fifteen, when he studied chemistry under Dalton in the rooms of the Society, continuing, by the most regular attendance at all the meetings, so long as his health permitted, and practically terminating with his death, must have been one of the most important circumstances of his life. Elected a member in the year of his greatest discoveries, 1842, he was Secretary in 1846, Vice-President in 1850, which office he held till his death, except during the ten years when he was President. He took the greatest interest in the welfare of the Society, and secured not only the veneration but love of all the members.

A man of science who left so deep a mark on his age ought to have been buried in Westminster Abbey, but unfortunately the necessary application could not be made, in consequence of the delay in the public announcement of his death. Prof. Osborne Reynolds, President of the Manchester Literary and Philosophical Society, has written to the *Times* urging that a monument should be erected in the Abbey, and that steps should immediately be taken to obtain, if possible, the consent of the Dean. This suggestion ought to meet with cordial and unanimous approval. Joule's name is one of which Englishmen may justly be proud, and the erection of a monument in Westminster Abbey would be the most fitting way in which they could express their appreciation of the splendour of his contributions to science.

[1] Man. Lit. and Phil. Soc., 1841-43; *Phil. Mag.*, ser. 3, vol. xix. p. 200.
[2] *Phil. Mag.*, ser. 3, vol. xxiii.

THE LIFE OF SIR WILLIAM ROWAN HAMILTON.

Life of Sir William Rowan Hamilton. By Robert Percival Graves, M.A. Vol. III. "Dublin University Press Series." (Dublin: Hodges, Figgis, and Co. 1889.)

At last the third and final volume of Graves's life of Sir William Rowan Hamilton has seen the light. It was our pleasure in former numbers of NATURE (vols. xxviii. p. 1, and xxxii. p. 619) to have reviewed the two earlier volumes, and we have now to congratulate the University of Dublin on the completion of an adequate biography of the most illustrious student that has ever issued from its halls. This present volume, of which we are now to speak, is as portly as its predecessors. It contains not less than 673 pages, and a very important

feature of the work is the correspondence between Hamilton and De Morgan, which occupies more than half of the entire bulk.

The two earlier volumes described the career of Hamilton up to the year 1854. The great mathematician was then forty-eight years old, and the first of his two stupendous books on quaternions had been just published for the admiration and astonishment of the scientific world.

The abundant recognitions of the epoch-marking nature of this work were naturally extremely gratifying to its author. From numerous scientific Societies at home and abroad honourable distinctions poured in upon him. But we can readily comprehend the biographer when he tells us that Hamilton specially prized the tributes from such friendly and competent judges as Sir John Herschel and a few others of similar calibre.

It must, however, be recorded that those who expressed, and no doubt felt such admiration for the book had not always, as they themselves admitted, any very complete acquaintance with the subject. Herschel, for instance, says:—

"I got through the first three chapters of it with a much clearer perception of meaning than when I attacked it some three or four years back, but I was again obliged to give it up in despair."

Even though Hamilton and his intimate friend De Morgan corresponded for twenty-five years—even though De Morgan was himself a capable mathematician, whose labours led him in some degree towards the same line of investigation from which quaternions originated—yet he never succeeded in obtaining a competent acquaintance with Hamilton's theory. Most scientific men were apt to feel contented if they knew enough to make them reverence the awful volume which every mathematician likes to see on his shelves, but which we generally find that he likes to leave there.

It must be admitted that the hopes which Hamilton entertained as to the utility of quaternions as a mathematical calculus have not yet been fully realized. It seems to be the essence of modern mathematics that it should partake of the nature of exploration. For the furtherance of many departments of mathematics which are now ardently cultivated, a general and all-embracing calculus is not found so useful as are especial methods particularly contrived to the subject in hand. We may cite as an example of what we mean Klein's lectures on the Ikosahedron, which were so admirably reviewed in a recent number of NATURE (May 9, p. 35). The theory here involved may be regarded as typical of all that is best in modern mathematics; yet the methods which have been adopted are not those of any comprehensive calculus like quaternions; they are processes and lines of reasoning which naturally arise from the special character of the subject, and the same may be said with regard to other branches of mathematical research. Of course everyone admits the magnificence of quaternions viewed as a mathematical theory. It is indeed "a tract of beautiful country"; but, for the reason, apparently, that we have mentioned, it would seem that up to the present, at all events, the hopes which Hamilton entertained that quaternions should be largely used by mathematicians as a calculus have not been realized.

As to the value of quaternions we quote the following words of Prof. Tait, which occur in the preface of his well-known treatise on the subject, for probably there is no other living mathematician who could speak with equal authority:—

"The numerous examples I have given, though not specially chosen so as to display the full merits of quaternions, will yet sufficiently show their admirable simplicity and naturalness to induce the reader to attack the lectures and the elements; where he will find, in profusion, stores of valuable results, and of elegant yet powerful analytical investigations, such as are contained in the writings of but a very few of the greatest mathematicians."

The extraordinarily laborious habits of Hamilton are constantly referred to in this volume. He frequently speaks of working for twelve consecutive hours at mathematical research, and when immersed in these trances of discovery his regular meal hours were forgotten, and, unfortunately, he used to resort unduly to alcoholic stimulant. His relaxations, such as they were, may be conjectured from the following paragraph (p. 186) from a letter addressed to Dr. (now Sir Andrew) Hart by Hamilton the year before he died:—

"The fact is, that one of my early tastes was for metaphysics, and something has lately occurred to revive it. Another was for Eastern languages, and I chanced yesterday to light on the first sheet of a 'Persian grammar' written by myself forty years ago. These things, with others, may occasionally relax the bow—'*Non semper tendit*' but 'many tastes one power,' and my only *power* is mathematics."

As we have already stated, both the study of poetry and the writing of poetry were favourite recreations with Hamilton; and as this subject is so frequently mentioned in these volumes it may be well to mention what the biographer—himself an excellent judge—records on the matter (p. 128):—

"I will not here refrain from stating my own opinion, strengthened by that of friends specially competent to judge, that Hamilton's poems have, both in their diction and in their matter, qualities of enduring value; that, speaking generally, they are and will always be felt to be fresh, graceful, fervid expressions of states of feeling and thought, interesting in themselves and possessing a heightened interest from their being the heart's utterances of a man of gigantic mathematical powers and of strong and deep affections; and a few of them, it may be added, are so happy in thought and expression as to claim their place in the poetry of his country."

The closing years of Hamilton's life were spent in the preparation and the passing through the press of his second great volume, "The Elements of Quaternions." In the summer of 1865, his stupendous labours had at last reached their termination. His health, which had for long been failing, now gave way, and at last, from a complication of ailments, which included gout and bronchitis, he died on the 2nd of September, 1865.

An interesting chapter on the "characterizations" of Hamilton and his work closes the biographical part of the volume. A letter from Prof. De Morgan to Lady Hamilton (p. 215) contains the following passage:—

"I have called him one of my dearest friends: for I know not how much longer than twenty-five years we have been in intimate correspondence of most friendly

agreement or disagreement, of most cordial interest in each other, and yet we did not know each other's faces. I met him about 1830 at Babbage's breakfast-table, and there for the only time in our lives we conversed. I saw him a long way off at the dinner given to Herschel about 1838, on his return from the Cape, and there we were not near enough, nor on that crowded day could we get near enough to exchange a word; and this is all I ever saw, and, so it has pleased God, all I shall see in this world, of a man whose friendly communications were among my greatest social enjoyments and greatest intellectual treats."

Hamilton was scrupulously sensitive about the feelings of others, and in his scientific work he took the most elaborate precautions that every particle of credit should be duly assigned to every mathematician whose labours had in the minutest extent anticipated his own. He had high personal courage and a keen sense of honour. It is recorded that he sent a hostile message to one who had challenged his veracity, and a due apology was exacted. The only approach that Hamilton ever had to a controversy about priority arose in the case of the discovery of conical refraction. MacCullagh put forward a claim to have virtually anticipated Hamilton. The sound judgment and lofty sense of right which were so characteristic of Humphrey Lloyd here averted what would have been an unpleasant dispute, and MacCullagh withdrew his pretension.

De Morgan says that in Hamilton's youth he used to be styled among his playmates the "defender of the absent," and from the same source we learn:—

"He relished the extremes both of simplicity and splendour; though in his own habits and manners as plain as possible, he thought much of the comforts of others and lightly of his own. When some housebreakers were caught on the premises and detained until they could be carried before a magistrate, he amused his family by directing that the fellows should be asked whether they preferred tea or milk for breakfast. A full memoir of his private and public life would present a genial combination of intellectual greatness, moral goodness, and piquant peculiarity of thought and manner, all brightened by never-ceasing benevolence of feeling, and toned by rare gentleness of manner."

Hamilton has left behind him an enormous bulk of manuscripts, of which not less than sixty great volumes have been deposited in the College Library. There are also many other papers unpublished, as, for instance, a stupendous letter to Dr. Hart, which contained about 240 large folio pages and a postscript of 60 additional.

The distinctive feature of this third volume of his life is the De Morgan correspondence, which, like the correspondence between Bessel and Olbers, will retain a permanent value. The opening letter of the series is from De Morgan, on May 8, 1841, in which he says: "I hardly know whether you remember that we made a little personal acquaintance some twelve years ago, when you were in London." The last is also from De Morgan, and the date February 3, 1865, relative to an application for a pension for the widow of the late Prof. Boole.

The correspondence, though largely on matters mathematical, ranges over a multitude of topics, classical and literary, logical and metaphysical, humorous and domestic. It sometimes glances at theology, occasionally condescends to refer to a practical question like the decimal system, but is almost wholly free from any reference to what are now generally known as the experimental or natural sciences, though we do indeed read of an electro-magnetic quaternion. From such an astounding mass of 400 pages it is difficult to cull passages which shall be regarded as samples. The collection is too heterogeneous to admit of such a process. It is, however, impossible not to feel how admirably the biographer has accomplished this part of his task. He has extracted from a mighty bulk of correspondence a collection so interesting that, after reading them through, we do not see any which we could wish to have been omitted.

It was three years after the interchange of letters had begun, and when Hamilton was thirty-nine years old, that De Morgan writes (p. 258):—

"I hope you are well, and taking care of yourself. Nobody gives you a good character in the second particular.

"The Astronomer-Royal in this country always lays down his work the moment he feels wrong, and plays till he feels right again. You have too much of our stock of science invested in your head to be allowed to commit waste. You are only tenant for life, and posterity has the reversion; and I don't see why you should not be compelled to keep yourself in repair."

Traditions of the state of Hamilton's study are still current. The awful masses of books and papers accumulated on the floor to such an extent that a lane had to be preserved for the perambulations of the Professor, who did much of his work standing at a blackboard, or walking up and down. Thus we can understand him writing to De Morgan on October 3, 1849 (p. 279):—

"If I lay a letter out of my hands for a few hours, without answering it, I am sure to find that it has been swept away and covered up, for the time, by the Charybdis of my other papers. No doubt, every such missing treasure may be expected, at some future time, to emerge to view, and may then be suddenly seized, by a bold and ready hand. Thus, from month to month, or at least from year to year, I find a note or two of yours eddying upward to the light; but, for the instant, your last long (and welcome) letter is invisible. However, I remember much of its contents, and shall send something now in answer to one, at least, of its 'loose thoughts.'"

As already mentioned, De Morgan could never be induced to devote himself sufficiently to quaternions to understand them. He often alludes to this, thus writing on October 11, 1849 (p. 283):—

"Nothing about quaternions will bore me, if I can only make it bore through me. Ink must be cheap in Ireland if you can afford to waste it on such a supposition as that."

Many amusing references are made by De Morgan to the contrast between his friendship to the Sir William Hamilton of Dublin, and his notorious controversies with the Sir William Hamilton of Edinburgh. Thus, on April 14, 1850 (p. 286):—

"Be it known unto you that I have discovered that you and the other Sir William Hamilton are reciprocal polars with respect to me (intellectually and morally, for the Scotch baronet is a Polar bear, and you, I was going to say, are a Polar gentleman, only I thought perhaps you might go and say I called you an Esquimaux). The intellectual polarity is of the kind, $\phi\psi(x) = -x$. When I send a bit of investigation to Edinburgh, the William

Hamilton of that ilk says I took it from him. When I send you one, you take it from me, generalize it at a glance, bestow it thus generalized upon society at large, and make me the second discoverer of a known theorem. He cuts my legs off; you make a pair of legs grow out of my head, and turn me upside down to stand upon them. His process after yours gives $\phi\psi(x) = -x$. Reciprocal polarity the last and most agreeable; your process involves no writing of pamphlets."

The gradual production of the books on quaternions are indicated by frequent allusions in the letters. On November 26, 1851, Hamilton writes (p. 291):—

"My book on the quaternions is advancing rapidly. I have just been correcting the slip which will bring it somewhat beyond 440 octavo pages. I first aimed at 200, but shall now congratulate myself if I get off under 500 pages. [It was ultimately 888.] You should be most welcome to copies of all the sheets hitherto printed, if I fancied that you would accept them, in the present state of the publication. In fact, I should *like* to send them, but think it not quite fair to *force* what may be thought a confidence on anyone."

To which De Morgan replies, in words that other authors will heartily appreciate (p. 293):—

"I beg you will send me the part printed, without scruple. There is a pleasure in reading while anything that strikes one may do service; it is the reviewer's feeling Christianized."

So much has been said about Hamilton's poems, and of the opinion he entertained of them, that it is well to read an authentic version of his own view of the matter in his letter of January 21, 1852 (p. 320):—

"If among your many and deep researches you have made psychology, as a sort of branch of natural history, one of them, you may feel some little interest in the following problem, which has often puzzled myself.

"Among the persons who know anything about my existence and my writings, I suppose that the majority would admit me to be a mathematician; while all, or nearly all, would say that I could only be regarded as a poet by courtesy.

"Does it not seem, then, to contradict one of the very tritest sayings about human nature that I care little, or not at all, about criticisms on my poetry, such as it is, while I own myself to be actually sensitive on the score of my mathematics?

"Wordsworth did me the honour to cut up, in a more slashing style than yours, some of my early poems. I think that I was less flattered than indifferent, although I did most highly prize the advantage of an intimacy with him.

"Slash away at my sonnets; but spare me, if you honestly can, a little praise for the quaternions, or, what will be far better, allow me the honour of *assisting* you to *use* them as a calculus, for such they certainly are."

Hamilton complains that he is ridiculed about the quaternions by people who know nothing of the method. De Morgan advises him to preface the book with a systematized body of results, on the ground that "every book should be provided with some palisade against mere talkers. . . . You will find the table of contents a useful outwork."

It would seem there was another class of quasi-scientific men of whom Hamilton, not without reason, had some dread, for we read (p. 360):—

"A story goes, that a person who read more than he digested once told a friend of his that he had heard people talk of 'Euclid,' and that he was curious to read the work. The other lent him a copy, and was surprised to find the borrower return it the next day with many thanks. 'What, have you read it?' said the lender. 'Yes, thank you,' replied the borrower. 'Read it?—read Euclid *through* in that short time?' 'Oh, if you mean the A's and the B's and the C's, I skipped all those.' The 'Quaternions' as well as the 'Formal Logic' may meet with some readers of that stamp."

To which De Morgan replies (p. 362):—

"When you tell the story about Euclid, remember that the man left out the A's and the B's and the C's and the pictures of scratches and scrawls. I once met a man, no strong mathematician, who said he read Airy's 'Gravitation' through on a bench in the front of his house."

We have learned in the earlier volumes of the almost miraculous talents which Hamilton displayed as a child for the acquisition of languages. It is therefore interesting to find him saying on April 16, 1852:—

"As to Sanscrit and Persian, I do not pretend to read them *now*; but my childish acquaintance with various languages may, as I have often since thought, have assisted me in my maturer study of mathematical symbols, and even in my attempts to enlarge the limits of mathematical expression."

An interesting letter to De Morgan on March 14, 1854, gives Hamilton's views on Auguste Comte's "Cours de Philosophie Positive," from which the following extract may be cited (p. 475):—

"These specimens, which I could easily multiply, may suffice to justify a profound distrust of Auguste Comte, wherever he may venture to speak as a mathematician. But his vast general ability, and that personal intimacy with the great Fourier which I most willingly take his own word for having enjoyed, must always give an interest to his *views* on any subject of pure or applied mathematics."

There were often long intermissions in the correspondence, generally on Hamilton's side, until at last De Morgan, by some genial and humorous attack, would succeed in reopening it. Witness the following:—

"If you are dead and buried, why can't you say so like a man, instead of leaving me to infer it from your silence."

And again on April 29, 1855 (p. 495):—

"Rouse out from the quaternions and write me a line. Remember that I, who have studied biography, and especially looked at the psychology of inventive mathematicians, do positively make affidavit, that if a man do not fallow, and shift courses, as they say, or used to say, in farming, he will put his head *out of heart*. If I had the power—supposing it true, as I hear, that you will not let quaternions alone for a while—I would put you into the commissariat, and make you copy out Balaklava stores, for six months. How are you all?"

These last lines remind us of a prescription recommended for another eminent mathematician, whose thoughts were apt to soar too far above terrestrial things. It was that he should serve for a couple of years as conductor to a London omnibus.

An appendix contains in the words of Hamilton himself what is designed to be as popular an account of the doctrine of quaternions as the subject will admit. It consists of two portions: firstly, an unfinished letter to his uncle, the Rev. James Hamilton, dated September 11

1846; the other an "elementary sketch," written after the publication of his volume of "Lectures on Quaternions." He here again insists on the combination of the notions of time and space in the quaternion.

> "And how the One of Time, of Space the Three
> Might in the Chain of Symbol girdled be."

The appendix also contains Hamilton's account of Mädler's attempt to show that the Pleiades are the central group of our sidereal system. The doctrine was unworthy of Hamilton's attention, and these pages are the only ones in this volume which we would gladly have seen omitted. Of infinitely more value are the remaining pages, in which we find a carefully compiled catalogue of all Hamilton's writings. There is also an interesting list of the chief unpublished manuscripts of Hamilton which have been preserved; and finally, before the capital index is reached, we have an enumeration, to which Prof. Tait has contributed, of the chief works of other mathematicians in which the quaternions have been employed.

It is impossible to read these volumes without acquiring a feeling of admiration amounting almost to reverence for the majestic intellect that they so adequately portray. In his youth Hamilton set before himself with deliberate aim the attainment of excellence in scientific work. Throughout his life he spared no pains, he flinched from no labour, to attain his ideal. With wonderful singleness and concentration he devoted his life to work. His career affords another illustration of the very close alliance if not actual identity between genius and the capacity for taking pains. If the quality of his work could not be surpassed, most assuredly its quantity could hardly be rivalled, and yet when we take leave of this work and ponder on the lessons it teaches it is perhaps hardly on the intellectual side of Hamilton's life that we shall find ourselves meditating. It is impossible to follow that exquisite correspondence between Hamilton and De Morgan without thinking of the rare but well-deserved good fortune which gave to De Morgan such a correspondent as Hamilton, which gave to Hamilton such a correspondent as De Morgan. On both sides these letters breathe a lofty spirit of truthfulness and honour and of attachment to whatsoever things are just and noble. Each shows a charming conception of the respect due to his friend and of the respect due to himself. Friendship such as these two men enjoyed was indeed a choice privilege. Hamilton shows himself not only as the consummate mathematician and philosopher, not only as the scholar and the poet, but as the high-minded gentleman with whom exalted conceptions of duty were habitual.

Finally, we must express our obligations to Mr. Graves for the admirable way in which he has completed his monumental task. He was intrusted with the preparation of the biography by Hamilton himself, and for laborious years he has devoted himself to the charge which his deceased friend laid upon him. Materials he had in abundance the most prodigal. He has selected copiously and he has selected judiciously, and he has told his wonderful story with a literary gracefulness that we most gratefully acknowledge. The memorable volumes of the "Lectures on Quaternions" and the "Elements of Quaternions" have a place on the shelves of all scientific libraries which are worthy of the name. Beside them should be ranged the three portly volumes in which Graves has recounted the life of that astonishing genius by whom Quaternions were invented.

THE ENGLISH TRANSLATION OF WEISMANN'S "ESSAYS."

Essays on Heredity and Kindred Biological Problems. By Dr. August Weismann. Authorized Translation, Edited by E. B. Poulton, Selmar Schönland, and Arthur E. Shipley. (Oxford: Clarendon Press, 1889.)

THIS is the fourth volume of a very useful series of translations of foreign biological memoirs, and the Delegates of the Clarendon Press are again to be congratulated on their choice of subject and editors.

As the editors' preface tells us, since Mr. Shipley's article, entitled "Death," in the *Nineteenth Century* for May 1885, first called the attention of English biologists to Prof. Weismann's essays, the interest in that author's conclusions and arguments has become very general. This interest has been stimulated and widened by the articles of various authors in NATURE, by Prof. Lankester's addresses at the Royal Institution, and, above all, by the great discussion introduced by Prof. Lankester at the meeting of the British Association in Manchester. No doubt a translation is superfluous, in these days of international science, to most biologists. But this is much more than a technical treatise on a technical subject. It is of interest to that far wider circle of readers and thinkers devoid of the time or the opportunity to wrestle with the involved German of the original. For to most the pith and kernel of the whole book is the criticism, perhaps the refutation, of the theory of the inheritance of acquired characters. That theory is of immediate importance to biology, but it is of equal, if remoter, importance to education and morality. We are certain that biologists do not enjoy a monopoly of education: we are by no means certain that they enjoy a monopoly of morality.

The translation is very accurate and unusually elegant: the foreign idiom has, to a large extent, been avoided, and there is little trace of the intricacies of Teutonic inversion. The footnotes are exceedingly useful, and many of them introduce important collateral matter. For instance, on p. 172, a concise and clear account is given of Mr. Francis Galton's earlier and somewhat parallel explanation of heredity.

The typography is excellent, but the absence of spaced type—a device of great utility in the original—is to be regretted.

The essays are translated in chronological order from the revised German editions. And so, by a consecutive perusal of the book, an historical conception of the wonderful series of inductions and inferences may be gained. Among the many converging lines of thought and work that led to the conception of the germ-plasma as the basis of heredity, two seem most clearly marked. In a prolonged study of the Hydromedusæ, Dr. Weismann discovered that the generative cells were formed only in certain localized areas. These special areas vary in position in different species, and the differences in position correspond to the different stages in a process of

displacement occurring in the phylogeny of the Hydroids. In the actual development these stages are repeated, and the primitive germ-cells migrate from the ancestral to the present position. From this it followed that the germ-cells contained something *sui generis:* something that could not be derived from the tissue-cells.

The first and third essays, on the other hand, show how a more or less theoretical consideration of death as a factor in biology led to the establishing of an actual continuity of life from individual to individual in genealogical series. In all animals above those consisting essentially of a single cell, this continuity of life is confined to the generative cells, and it is the other, or somatic, cells alone that are necessarily mortal.

Such converging lines led to the provisional hypothesis of a continuity of germ-plasma as the basis of heredity—the hypothesis in fact, to take a simple instance, that it is the eggs that have been forming the hens, and not the hens the eggs, and so with their ancestors from the remotest of times. With this new view came the discussion of the inheritance of acquired characters and the brilliant interpretations and investigations of parthenogenesis and polar bodies. Essay VII., on the supposed botanical proofs of the transmission of acquired characters—which has not before appeared in any form in English—and Essay VIII., on the supposed transmission of mutilations, are valuable contributions to the questions raised by the general theory.

There can be no doubt but that Dr. Weismann's essays will be for long a source of inspiration and stimulus to supporter and adversary, and this valuable translation must prove of great service in making better known what, if it never advances beyond the stage of a provisional hypothesis, has already been of the utmost service to biology.

P. C. M.

OUR BOOK SHELF.

Chambers's Encyclopædia. New Edition. Vol. IV. (London and Edinburgh: W. and R. Chambers, 1889.)

IN this volume of the new edition of "Chambers's Encyclopædia," subjects from "Dionysius" to "Friction" are dealt with. So far as we have been able to test it, we have found that the volume is in no respect inferior to its predecessors. The subjects include some that are of great scientific interest and importance, and these have been intrusted to writers whose names are a sufficient guarantee for the character of their work. Prof. Tait writes the article on force, Dr. W. Peddie those on energy and ether, and Prof. Cargill G. Knott that on electricity. Dynamos, the electric light, and the electric railway are described by Prof. J. A. Ewing. The theory of evolution is presented by Prof. Patrick Geddes, who, while expounding his own doctrine, tries to give a perfectly fair account of the opinions of thinkers with whom he only in part agrees. Dr. H. R. Mill has a good article on the earth, and Prof. James Geikie discourses with his usual clearness on Europe and on earthquakes. To the article on France, Prince Kropotkin contributes the geographical section. Prof. A. H. Keane is the author of the article on ethnology; and Dr. Henry Rink has a short but interesting paper on the Eskimo. These and other articles on scientific subjects in the present volume cannot fail to maintain the high reputation of "Chambers's Encyclopædia" for accuracy and thoroughness.

Farm Live Stock of Great Britain. By Robert Wallace, Professor of Agriculture at the University of Edinburgh. Second Edition. (Edinburgh: Oliver and Boyd. London: Simpkin, Marshall, and Co. 1889.)

THIS is a second edition of a work already reviewed in NATURE. The most important point of difference between it and the first edition is the introduction of 100 excellent plates, executed by Angerer and Göschl, of Vienna, from photographs taken from life. Pictures are, no doubt, of great assistance to a description, but, as the author justly observes, photographs, although accurate, fail in some respects to do justice to animals. This he attributes to the awkward positions they assume while standing, and the constancy of their motion while they remain on their limbs. It is also, no doubt, partly due to the higher elevation of the eye of the observer than the camera as usually employed. The levelness of the back and of the belly lines is destroyed by the camera when placed horizontally so as to strike the broadside of the animal. Prominences are shown against the light, which in ordinary observation do not disturb the levelness of the carcass. The work has a strictly pastoral and agricultural interest.

Days with Industrials; or, Adventures and Experiences among Curious Industries. By Alexander H. Japp, LL.D. (London: Trübner and Co., 1889.)

THIS book is a reprint, with additions, of a number of articles which have appeared from time to time in various periodicals of a popular character. The articles deal with such subjects as quinine, rice, pearls, amber, common salt, Burton ale and Dublin stout, petroleum, canaries, bedsteads, railway-whistles, knives, forks, and postage-stamps—as heterogeneous a mixture, in fact, as the contents of Mrs. Jellaby's famous cupboard. Dr. Japp writes in a chatty and agreeable style, and his book may be safely given to young people, with the certainty that they will imbibe no false notions of science.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Lamarck *versus* Weismann.

I HAD not intended to reply to Mr. Cunningham's criticism of a passage in my book which he thinks is pure Lamarckism (see NATURE, July 25, p. 297); but now that Prof. Ray Lankester adopts the same view, I will make a few remarks upon the case. Mr. Cunningham italicizes the words, "the constant repetition of this effort causes the eye gradually to move round the head till it comes to the upper side," and claims this as a Lamarckian explanation. But if we italicize the following words, which occur three lines further on, "*those usually surviving whose eyes retained more and more of the position into which the young fish tried to twist them,*" we shall see that the survival of favourable variations is, even here, the real cause at work. For the transference of the eye to the upper side was a useful change—perhaps, under the peculiar conditions of existence and development—an absolutely essential one. The amount to which the eye could be twisted and retained in its new position was variable, as all other such characters are variable. Those individuals who had this faculty in the greatest degree were among those that survived, and it is not at all necessary to assume that any portion of the change *due solely to the effort* was inherited, but only that those individuals which were the most favourably constituted in this respect transmitted their peculiar constitution to their offspring, and thus the twisting would take place earlier and earlier in the development of the individual Even Darwin himself, who believed in the heredity of acquired variations, says that "the *tendency to distortion* would no doubt be increased through the principle of inheritance"; and this is really all that is necessary. In most of the higher animals sym-

metrical development of the two sides of the body is of vital importance, and is strictly preserved by natural selection; but more or less defect of symmetry often occurs as a variation or monstrosity, and in cases where such asymmetry was useful these variations would be preserved and increased by selection and heredity. An altogether erroneous view is taken of the fact of effort being used in this case, as if it were something unusual. But in all cases selection produces changes which are useful and whose use is often indicated by effort. The giraffe uses effort in stretching its neck to obtain food during a drought; the antelope exerts itself to the utmost to escape from the leopard; but it is now recognized that it is not the individual change produced by this *effort* that is inherited, but the favourable constitution which renders extreme effort unnecessary, and causes its possessors to survive while those less favourably constituted, and who therefore have to use greater effort, succumb. In the case of the developing flat-fish also, the effort indicated the direction of the useful modification, and any variations tending either to the right kind of asymmetry or to a mobility of the eye, admitting its being twisted and retained in its new position, during the growth of the individual, would be certainly preserved.

I wish to take this opportunity of thanking Prof. Ray Lankester for his careful and appreciative review of my book. I am too well aware of my own deficiency in training as a naturalist not to admit all the shortcomings which he so tenderly refers to. It is quite refreshing to me to read at last a real criticism from a thoroughly competent writer, after the more or less ignorant praise which the book has hitherto received. I admit also that the term "laboratory naturalist," to which he demurs, was not well chosen. I meant it as the opposite, not so much to "field naturalist" as to "systematic naturalist"; and it still seems to me that the gentlemen he refers to as not being "laboratory naturalists" are still less "systematic naturalists," in the sense of having specially devoted themselves to the observation, description, and classification of more or less extensive groups of species of living organisms. ALFRED R. WALLACE.

A Mechanical Illustration of the Propagation of a Sound-Wave.

HAVING to prepare some lectures on sound, I wished, if possible, to illustrate, without any very complicated apparatus, the way in which a sound-wave is propagated.

The following method suggested itself to me. As I have not met with the method while examining a large number of works on sound and wave motion, I venture to send a description of it to NATURE, as it may perhaps be of use to some students of acoustics.

A row of pendulums of equal length, *a*, *b*, *c* . . . *l* (Fig. 1) are suspended from a rod AB; in order to start the pendulums,

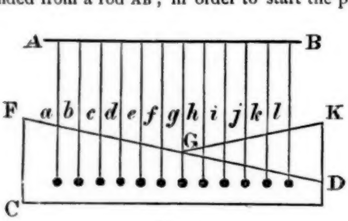

FIG. 1.

the bobs are held against an angular-shaped board, FCD, the rod being held in a plane slightly behind the plane of the board; if now the rod and pendulums be raised together vertically, *l* will first swing, then *k*, and so on, till all are free: when the pendulums are raised with a uniform velocity, then each pendulum starts at an equal period of time after the one which is next to it; the result is that a wave-motion is seen to run along the line of bobs as they vibrate to and fro. Such an arrangement has been used to illustrate wave-motion, as each bob moves with harmonic motion. But such an arrangement does not illustrate directly those compressions and rarefactions whereby sound is propagated. A slight movement, however, of the rod at once makes it do so. If, while the pendulums are vibrating, the rod from which they are suspended be turned in the horizontal plane through a right angle, the direction of the swing of each pendulum is not changed, and all the pendulums swing in the same plane. This will become clear from (Fig. 2), where the pendulum bobs viewed along OX appear to trace out wave-motion; the relative position of the bobs after the rod which supports them is turned through a right angle is shown along OY; the motion then illustrates mechanically those movements of air-particles which, when in compression and rarefaction, propagate a sound-wave. If the rod be turned back through a right angle, the wave-motion is again restored. The illustration must be taken with the obvious defect, viz. that the bobs move in arcs, and not in straight lines.

Care should be taken that the amplitude of vibration be not greater than the distance between the points of suspension minus

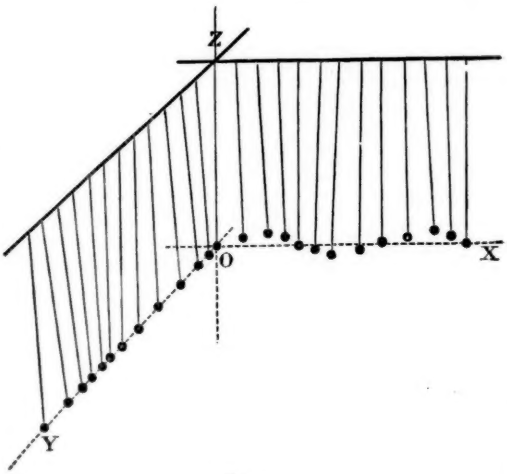

FIG. 2.

the diameter of a bob, otherwise the bobs will hit each other when vibrating in the plane YZ.

Twelve pendulums made of lead bullets 1·5 centimetre in diameter, suspended from threads 30 centimetres long, with a distance between each of 5 centimetres, were found to answer well by the author.

If the board used for starting the pendulums be made of the angular shape, FGK, then the movement of the bobs in their second position illustrates the propagation of sound on each side of its origin. FREDERICK J. SMITH.

Trinity College, Oxford, October 1.

On some Effects of Lightning.

THE twisting of one of the two trees near St. Albans, which were struck in such a remarkable manner by lightning, may well have been caused by the fall of the top of the tree, as Mr. Griffith suggests, and not directly by the lightning.

I have been unsuccessful in ascertaining whether the core of the tree is situated nearer that side where the explosion seems to have been most violent; but a more detailed examination only enforces the conclusion which Mr. Griffith and I arrived at, that the explosion must have occurred inside the stem, if not actually at the core of the tree.

The effects in this case can meet with no explanation from the supposition that the lightning passed between the bark and the tree, generating thereby sufficient steam to blow off the bark and shatter the stem—an explanation which Mr. Maclear suggests in his letter of September 25. I doubt if any source of heat would ever convert water so quickly into steam as to endow it with the power which dynamite has of shattering a hard object lying in contact with it, while the gases formed are restrained by the comparatively feeble resistance of the bark and outer air; nor can we suppose that sufficient heat could pass into the stem to generate steam there adequate for such an explosion, even if the uncharred condition of the wood did not prove incontestably that the temperature had not been raised very high. It seems more probable to me that such explosions must be caused by the lightning electrolysing the liquids in the stem, and

thereby causing the sudden production of large volumes of gases at the ordinary temperature.

In answer to Mr. Griffith's query, I may state that the two trees are 34 yards apart; that there is no other tree in a direct line between them, though there are two about 4 yards from this line, and about midway between them; that the trees are certainly not isolated in any way, since there are fifteen trees within less than 34 yards of one of them, and about the same number within the same distance of the other.

48 Bryanston Square. SPENCER PICKERING.

Yew Trees in Berks.

A COMMUNICATION from Mr. Walter Money respecting two yew trees which were planted in the churchyard of Basildon, Berks, by Charles, Lord Fane, in 1726, has appeared lately in some of the papers (*North Wilts Herald*, October 4, 1889, and *Standard*), in which he refers to the dimensions recorded in the parish register, taken in 1780 and 1796, and again by my father in 1834. He adds his own observations on the growth taken this year. As I happen to have the original notes made by the late Prof. J. S. Henslow, dated "1834, August," it may be not uninteresting to record them. He writes as follows:—

"Measurements of yew trees at Basildon Churchyard, planted in 1726; taken near the ground:

			Ft.	In.	
Tree to south,	1780	... ...	6	3	According to Register.
	1796	... ...	8	6	
	1834	... ...	8	9	[J. S. H.]
	,,	(at 4 feet)	6	10½	
	[1889	... ...	9	10	Mr. Money]

Tree to north [1780 and 1796 not recorded in register]:

			Ft.	In.	
Roots lately injured by digging graves.	1834	... ...	9	2½	[J. S. H.]
	,,	(at 4 feet)	6	9¾	
	[1889	... ...	9	6	Mr. Money]

"From the three observations of 1780, 1796, and 1834, it would appear that the period of rapid growth stopped about 1796; but it seems probable that the measurement here is somewhat too great compared with that of 1780, as well as with that of 1834; for

Growth to		Ft.	In.		Lines.		Lines.			Years.	Lines per ann.
1780	...	6	3	=	900	gives	300	diam. of gr.	in	54,	*i.e.* 5½
1796	...	2	3	=	324	,,	108	,,	,,	16,	,, 6¾

"Allowing this measurement to be wrong by 6 inches, it will reduce it to a greater probability also with that of 1834; and we shall have—

Growth to		Ft.	In.		Lines.		Lines.			Years.	Lines per ann.
1796	...	1	9	=	252	gives	84	diam. of gr.	in	16,	*i.e.* 5¼
1834	...		9	=	108	,,	36	,,	,,	38,	,, 1
[1889	...	1	1	=	156	,,	52	,,	,,	55,	,, 1]

"N.B.—The increase between 1780 and 1796 is too great, supposing the same parts to have been measured; and between 1796 and 1834 it is too little; therefore 1796 either took in too much of the circumference of the roots, or perhaps 1780 a little above them. Possibly the soil had become somewhat raised since 1796.

[Since 1834 the growth for the last fifty-five years will be seen to be exactly the same per annum, or 1 line.]

[With regard to the rate of increase at a height of 4 feet from the ground, he adds the following additional note.]

"Now the rate of increase of 4 feet from the ground is slower than that near the root, upon the whole, in the proportion of one-fourth, nearly. Taking, therefore, this fact with the indications given above, we may average the growth of the stem at 4 feet in the following way:—

"Diameter at 4 feet $= \frac{990}{3}$ lines $=$ 330 (in 1834). Dividing this by the age, or 108 years, it gives 3 lines per annum nearly.

"Also 1780 to 1796 gives 84 lines for 16 years, *i.e.* 5 lines per annum.

"As it seems not to have grown much in the last twenty-eight years (*i.e.* up to 1834), if we allow 1 line for this period, and distribute for the eighty years of rapid growth, we get the following result; thus:—

First 20 years	at	3½ lines	=	70	Young growth.
,, 40	,,	4 ,,	=	160	
,, 20	,,	3 ,,	=	60	
,, 28	,,	1½ ,,	=	42	Settled period."

GEORGE HENSLOW.

Maxwell's "Electricity and Magnetism."

THERE is apparently a trifling slip in § 360 of Maxwell's "Electricity and Magnetism." The ratio of the resistance of pure iron at 100° C. to the resistance at 0° C. is there stated to be 1·645. This ratio is evidently calculated from the results given in Matthiesen's paper on the influence of temperature on the electric conducting power of thallium and iron (Proc. Roy. Soc., 1862–63). The true ratio for pure iron annealed in hydrogen is 1·6255. The other ratios mentioned in the paragraph are correctly deduced.

HERBERT TOMLINSON.

King's College, Strand, October 12.

AN EXAMINATION OF SOME POINTS IN PROF. WEISMANN'S THEORY OF HEREDITY.[1]

PROF. WEISMANN'S views on heredity and allied phenomena have met with such general acceptance that I feel it to be presumptuous on my part to attempt any criticism of them. I cannot but think, however, that a statement of the difficulties which they present to me, and of the inconsistencies which appear to exist in the argument, may be of value, not indeed as a refutation, but as drawing attention to those points which seem to require further elucidation.

It will be necessary for me to state Prof. Weismann's argument, and I shall endeavour, in so doing, to represent it as fully and as fairly as my apprehension of it will admit, and as far as possible in his own words. But this is a matter of no small difficulty, inasmuch as the argument has to be traced through a number of separate essays, even though these essays have been collected into one volume and translated into English. All the references which I make relate to the English edition.[2]

The fundamental fact upon which the whole argument is based, and which Prof. Weismann appears to have fully established, is that the body of unicellular organisms (monoplastides), as also that of undifferentiated multicellular organisms (homoplastides), is immortal, at any rate potentially. This position is clearly stated in the following passage (p. 25):—

"The process of fission in the Amœba has been recently much discussed, and I am well aware that the life of the individual is generally believed to come to an end with the division which gives rise to two new individuals, as if death and reproduction were the same thing. But the process cannot truly be called death. Where is the dead body? What is it that dies? Nothing dies; the body of the animal only divides into two similar parts, possessing the same constitution."

Death is, on the contrary, a characteristic feature of differentiated multicellular organisms (heteroplastides); but even in these forms there is still an immortal part, for the reproductive cells which develop into new individuals are evidently as potentially immortal as the Amœba. In these higher organisms, therefore, the mortal cells are to be distinguished from the immortal. This distinction is drawn by Prof. Weismann as follows (p. 122):—

"It is necessary to distinguish between the mortal and the immortal part of the individual—the body in its narrow sense (*soma*) and the germ-cells. Death only affects the former; the

[1] This paper is an expansion of some remarks contributed to the discussion on "The Transmission of Acquired Characters," which took place in Section D during the recent meeting of the British Association at Newcastle.

[2] Weismann, "On Heredity" (Oxford: Clarendon Press, 1889).

germ-cells are potentially immortal, in so far as they are able, under favourable circumstances, to develop into a new individual, or, in other words, to surround themselves with a new body (*soma*)." (See also p. 158.)

This statement is further explained on p. 205:—

"Strictly speaking, I have therefore fallen into an inaccuracy in maintaining (in former works) that the germ-cells are themselves immortal; they only contain the undying part of the organism—the germ-plasm; and although this substance is, as far as we know, invariably surrounded by a cell-body, it does not always control the latter, and thus confer upon it the character of a germ-cell."

Similarly, the substance of the body is termed somatoplasm (p. 104). Moreover, the germ-plasm is stated to be localized in the nucleus of the germ-cell (p. 179).

The first difficulty which presents itself is to understand how the mortal heteroplastides can have been evolved from the immortal monoplastides or homoplastides. The explanation which Prof. Weismann offers is as follows (p. 27):—

"Let us now consider how it happened that multicellular animals and plants, which arose from unicellular forms of life, came to lose this power of living for ever.

"The answer to this question is closely bound up with the principle of division of labour which appeared among multicellular organisms at a very early stage, and which has gradually led to the production of greater and greater complexity in their structure.

"The first multicellular organism was probably a cluster of similar cells, but these units soon lost their original homogeneity. As the result of mere relative position, some of the cells were especially fitted to provide for the nutrition of the colony, while others undertook the work of reproduction. Hence the single group would come to be divided into two groups of cells, which may be called somatic and reproductive—the cells of the body as opposed to those which are concerned with reproduction. This differentiation was not at first absolute, and indeed it is not always so to-day. Among the lower Metazoa, such as the Polypes, the capacity for reproduction still exists to such a degree in the somatic cells, that a small number of them are able to give rise to a new organism—in fact, new individuals are normally produced by means of so-called buds. Furthermore, it is well known that many of the higher animals have retained considerable powers of regeneration; the salamander can replace its lost tail or foot, and the snail can reproduce its horns, eyes, &c.

"As the complexity of the Metazoan body increased, the two groups of cells became more sharply separated from each other. Very soon the somatic cells surpassed the reproductive in number, and during this increase they became more and more broken up by the principle of division of labour into sharply separated systems of tissues. As these changes took place, the power of reproducing large parts of the organism was lost, while the power of reproducing the whole individual became concentrated in the reproductive cells alone."

It is clear that this explanation, plausible as it seems to be, leaves untouched the real question at issue; the question as to how mortal cells could have been evolved from immortal. Prof. Weismann himself seems to have been conscious of this, for on p. 139 he reverts to the subject as follows:—

"It may be objected that cells of which the ancestors possessed the power of living for ever, could not have become potentially mortal (that is, subject to death from internal causes) either suddenly or gradually, for such a change would contradict the supposition which attributes immortality to their ancestors, and to the products of their division. This argument is valid, but it only applies so long as the descendants retain their original constitution. But as soon as the products of the fission of a potentially immortal cell acquire different constitutions by unequal fission, another possibility arises. Now it is conceivable that one of the products of fission might preserve the physical constitution necessary for immortality, but not the other, just as it is conceivable that such a cell—adapted for unending life—might bud off a small part, which would live a long time without the full capabilities of life possessed by the parent cell; again, it is possible that such a cell might extrude a certain portion of organic matter (a true excretion) which is already dead at the moment it leaves the body. Thus it is possible that true unequal cell-divisions, in which only one half possesses the condition necessary for increasing, may take place; and in the same way it is conceivable that the constitution of a cell determines the fixed duration of its life, examples of which are before us in the great number of cells in the higher Metazoa, which are destroyed by their functions. . . . But the reproductive cells cannot be limited in this way, and they alone are free from it. They could not lose their immortality, if indeed the Metazoa are derived from the immortal Protozoa, for from the very nature of that immortality it cannot be lost. From this point of view the body, or *soma*, appears in a certain sense as a secondary appendage of the real bearer of life—the reproductive cells."

Prof. Weismann here comes to closer quarters with the real question at issue, but still he does not fully face it. He invokes the principle of "unequal fission" to account for the acquisition of "different constitutions" by the products of fission, but he offers no explanation whatever of the *modus operandi* of unequal fission. He makes no suggestion as to the constitution of the body of the Protozoa; whether it consists, in his opinion, entirely of germ-plasm, and if not, whether or not the germ-plasm is localized in the nucleus. The only criticism which can be made is that the bare mention of "unequal fission" is not a sufficient answer to the objection "that cells, of which the ancestors possessed the power of living for ever, could not have become potentially mortal." It appears to me that any satisfactory answer to this objection must include the assumption that the immortal ancestors already contained a substance which was potentially mortal. It is impossible to conceive that unequal fission can take place in a cell consisting throughout of essentially the same kind of substance.

Very much the same difficulty presents itself in connection with the development of the embryo from the ovum or germ-cell; in the one case it is phylogenetic, in the other ontogenetic. Prof. Weismann goes into far greater detail in this latter case, and the statements which he makes concerning it may perhaps be intended to throw some light on the former.

The germ-cell, as pointed out above, is characterized by containing germ-plasm; and this germ-plasm is localized in the nucleus. There is one point which Prof. Weismann does not mention, and that is as to the nature of that portion of the germ-cell (including the cytoplasm and part of the nucleoplasm) which does not consist of germ-plasm. Of what, then, does it consist? It must consist of somatoplasm: there is no alternative. The germ-cell, then, consists mainly of mortal somatoplasm, and contains in its nucleus a certain amount of immortal germ-plasm. But, as shown in preceding quotations, Prof. Weismann holds that the whole germ-cell is immortal. In view of the constitution of the germ-cell, this view seems to be paradoxical, but it appears to be explained on the assumption that the substance of the nucleus determines the nature and character of the cell, though Prof. Weismann does not altogether commit himself to this assumption (see pp. 185, 205, 210).

From this point of view Prof. Weismann's suggestion that the development of mortal from immortal cells is due to unequal fission, seems to be quite intelligible, not only ontogenetically, but also phylogenetically, if we venture to assume that the constitution of a Protozoon is essentially the same as that of a germ-cell. It is easy to imagine that the nucleus of a Protozoon may be divided into two parts, one of which contains the whole or the greater part of the parental germ-plasm, the other containing none or only a small portion of it; the two resulting cells would be respectively immortal and mortal, and, supposing they remained coherent, would represent the reproductive and somatic portions of a heteroplastid body. Similarly, if such a division of the nucleus of the

germ-cell took place, the two resulting cells would represent the reproductive and somatic portions of the body of the embryo.

But this does not appear to be Prof. Weismann's view of embryogeny. On the contrary, he holds strongly that the germ-plasm of the ovum gives rise, in part at least, to the somatoplasm of the embryo. Thus, on p. 168 he says:—

"I have called this substance 'germ-plasm,' and have assumed that it possesses a highly complex structure, conferring upon it the power of developing into a complex organism. I have attempted to explain heredity by supposing that in each ontogeny a part of the specific germ-plasm contained in the parent egg-cell is not used up in the construction of the body of the offspring, but is preserved unchanged for the formation of the germ-cells of the following generation."

It is not a little remarkable that Prof. Weismann should not have offered any suggestion as to the conception which he has formed of the mode in which the conversion of germ-plasm into somatoplasm can take place, considering that this assumption is the key to his whole position. He has been at considerable pains to controvert the view that somatoplasm may be converted into germ-plasm; but in making this attack he has overlooked the necessity for defence. There is really no other criticism to be made on an unsupported assumption such as this, than to say that it involves a contradiction in terms. The idea of the conversion of germ-plasm into somatoplasm is quite as impossible as that of the conversion of somatoplasm into germ-plasm. It is absurd to say that an immortal substance can be converted into a mortal substance. If such an apparent change takes place, the only possible conclusion is that the so-called immortal substance was never truly immortal, inasmuch as it must have always possessed the potentiality of mortality.

It may perhaps be represented that the foregoing criticisms are altogether of too minute and detailed a character to affect the general validity of Prof. Weismann's argument. My answer is that I understand Prof. Weismann to imply that his theory of heredity is not—like, for instance, Darwin's theory of pangenesis—"a provisional or purely formal solution" (Weismann, p. 166) of the question, but one which is applicable to every detail of embryogeny, as well as to the more general phenomena of heredity and variation.

We may now proceed to the consideration of Prof. Weismann's theory of heredity. The essential features of it are given in the following paragraphs (p. 73):—

"Among these unicellular organisms, heredity depends upon the continuity of the individual during the continual increase of its body by means of assimilation.

"But how is it with the multicellular organisms which do not reproduce by means of simple division, and in which the whole body of the parent does not pass over into the offspring?

"In such animals the power of reproduction is connected with certain cells, which, as germ-cells, may be contrasted with those which form the rest of the body; for the former have a totally different *rôle* to play; they are without significance for the life of the individual (that is, for the preservation of its life), and yet they alone possess the power of preserving the species. Each of these can, under certain conditions, develop into a complete organism of the same species as the parent, with every individual peculiarity of the latter reproduced more or less completely. How can such hereditary transmission of the characters of the parent take place? How can a single reproductive cell reproduce the whole body in all its details?"

Prof. Weismann's answer to these questions is as follows:—

"We have an obvious means by which the inheritance of all transmitted peculiarities takes place, *in the continuity of the substance of the germ-cells, or germ-plasm*. If, as I believe, the substance of the germ-cells, the germ-plasm, has remained in perpetual continuity from the first origin of life, and if the germ-plasm and the substance of the body, the somatoplasm, have always occupied different spheres, and if changes in the latter only arise when they have been preceded by corresponding changes in the former, then we can, up to a certain point, understand the principle of heredity; or, at any rate, we can conceive that the human mind may at some time be capable of understanding it" (p. 104).

"Now if it is impossible for the germ-cell to be, as it were, an extract of the whole body, and for all the cells of the organism to despatch small particles to the germ-cells, from which the latter derive the power of heredity; then there remain, as it seems to me, only two other possible, physiologically conceivable, theories as to the origin of germ-cells, manifesting such powers as we know they possess. Either the substance of the parent germ-cell is capable of undergoing a series of changes which, after the building-up of a new individual, leads back again to identical germ-cells; or the germ-cells are not derived at all, as far as their essential and characteristic substance i concerned, from the body of the individual, but they are derived directly from the parent germ-cell.

"I believe that the latter view is the true one. . . . . propose to call it the theory of the 'continuity of the germ plasm,' for it is founded upon the idea that heredity is brough about by the transference, from one generation to another, of a substance with a definite chemical, and, above all, molecula constitution. I have called this substance 'germ-plasm,' and have assumed that it possesses a highly complex structure, conferring upon it the power of developing into a complex organism. I have attempted to explain heredity by supposing that in each ontogeny a part of the specific germ-plasm contained in the parent egg is not used up in the construction of the body of the offspring, but is reserved unchanged for the formation of the germ-cells of the following generation" (p. 167).

"I believe that heredity depends upon the fact that a small portion of the effective substance of the germ, the germ-plasm, remains unchanged during the development of the ovum into an organism, and that this part of the germ-plasm serves as a foundation from which the germ-cells of the new organism are produced. There is, therefore, continuity of the germ-plasm from one generation to another. One might represent the germ-plasm by the metaphor of a long creeping root-stock from which plants arise at intervals, these latter representing the individuals of successive generations" (p. 266).

This theory appears to fully account for the transmission and maintenance of ancestral characters; but of course it depends on the assumption that the germ-plasm is a substance of great stability. This is, in fact, Prof. Weismann's view (p. 271):—

"The germ-plasm, or idioplasm of the germ-cell (if this latter term be preferred), certainly possesses an exceedingly complex minute structure, but it is nevertheless a substance of extreme stability, for it absorbs nourishment, and grows enormously without the least change in its complex molecular structure."

In spite of the simple, and apparently satisfactory, explanation of the phenomena of heredity which this theory affords, there are, nevertheless, serious difficulties in the way of its acceptance. It is open to criticism even from Prof. Weismann's own standpoint. The fate of the germ-plasm of the fertilized ovum is, according to Prof. Weismann, to be converted in part into the somatoplasm of the embryo, and in part to be stored up in the germ-cells of the embryo. This being so, how are we to conceive that the germ-plasm of the ovum can impress upon the somatoplasm of the developing embryo the hereditary character of which it (the germ-plasm) is the bearer? This function cannot be discharged by that portion of the germ-plasm of the ovum which has become converted into the somatoplasm of the embryo, for the simple reason that it has ceased to be germ-plasm, and must therefore have lost the properties characteristic of that substance. Neither can it be discharged by that portion of the germ-plasm of the ovum which is aggregated in the germ-cells of the embryo, for under these cumstances it is withdrawn from all direct relation with the developing somatic cells. The question remains without an answer.

Still more is the theory open to criticism from the standpoint which I have established above. It is clear that the theory of the continuity of the germ-plasm, as explaining heredity, is only valid on the assumption that the germ-plasm of the ovum gives rise to the somatoplasm of the embryo. But I have shown above that the conversion of germ-plasm into somatoplasm is inconceivable; and, even if it be admitted, it cannot be seriously maintained that the whole body of the embryo is, in any case, developed solely from the germ-plasm of the ovum. On the contrary, since the embryo is developed from the *whole* of the nucleus and more or less of the cytoplasm of the ovum, it must be admitted that the non-germ-plasm, or somatoplasm, of the ovum provides a large part of the material in embryogeny. It is an obvious inference that, under these circumstances, hereditary characters may be transmitted from the parent to the offspring, not only by the germ-plasm, but also by the somatoplasm, of the ovum.

It might be replied to these criticisms that, even if it be admitted that germ-plasm cannot be converted into somatoplasm, and also that the somatic cells of the embryo are derived from the somatoplasm of the ovum, it is still conceivable that the nuclei of the somatic cells of the embryo may contain a certain amount of the germ-plasm of the ovum, not enough to confer upon the somatic cells the properties of germ-cells, but sufficient to determine their growth and differentiation in accordance with the hereditary tendencies of which the germ-plasm is the bearer. But this view does not appear to be held by Prof. Weismann, whose opinion with reference to the presence of germ-plasm in somatic cells is as follows (p. 211):—

"I believe I have shown that theoretically hardly any objections can be raised against the view that the nuclear substance of somatic cells may contain unchanged germ-plasm, or that this germ-plasm may be transmitted along certain lines. It is true that we might imagine *a priori* that all somatic nuclei contain a small amount of unchanged germ-plasm. In Hydroids such an assumption cannot be made, because only certain cells in a certain succession possess the power of developing into germ-cells; but it might well be imagined that in some organisms it would be a great advantage if every part possessed the power of growing up into the whole organism, and of producing sexual cells under appropriate circumstances. Such cases might exist if it were possible for all somatic nuclei to contain a minute fraction of unchanged germ-plasm."

After alluding to the fact that new plants can be developed from leaves of Begonia which have been cut off and laid in moist sand, Prof. Weismann continues:—

"But I think that this fact only proves that, in Begonia and similar plants, all the cells of the leaves, or perhaps only certain cells, contain a small amount of germ-plasm, and that, consequently, these plants are specially adapted for propagation by leaves. How is it, then, that all plants cannot be reproduced in this way? No one has ever grown a tree from a leaf of the lime or oak, or a flowering plant from the leaf of the tulip or convolvulus. It is insufficient to reply that, in the last-mentioned cases, the leaves are more strongly specialized, and have thus become unable to produce the germ-substance; for the leaf-cells in these different plants have hardly undergone histological differentiation in different degrees. If, notwithstanding, the one can produce a flowering plant, while the others have not the power, it is of course clear that reasons other than the degree of histological differentiation must exist; and, according to my opinion, such a reason is to be found in the admixture of a minute quantity of unchanged germ-plasm with some of their nuclei."

It appears, therefore, to be Prof. Weismann's opinion that it is only in special cases that germ-plasm is present in somatic cells, and that, when present, it confers on the somatic cells the properties of germ-cells, though it is difficult to reconcile this opinion with the following statement on p. 205:—

"Strictly speaking, I have therefore fallen into an inaccuracy in maintaining (in former works) that the germ-cells are themselves immortal; they only contain the undying part of the organism—the germ-plasm; and although this substance is, as far as we know, invariably surrounded by a cell-body, it does not always control the latter, and thus confer upon it the character of a germ-cell."

I would submit, therefore, that, inasmuch as Prof. Weismann offers no evidence to prove the continuity of the germ-plasm of the ovum with the somatoplasm of the embryo, his principle of the continuity of the germ-plasm cannot be regarded as a satisfactory theory of heredity; and I would point out that the facts of embryogeny seem to confer upon the idea of a continuity of the somatoplasm at least as high a degree of probability as upon that of a continuity of the germ-plasm.

We come, finally, to Prof. Weismann's explanation of variation, a connected statement of which is to be found on pp. 277 *et seq.*, from which I may make the following quotations:—

"The origin of hereditary individual variability cannot indeed be found in the higher organisms—the Metazoa and Metaphyta; but it is to be sought in the lowest—the unicellular organisms. In these latter the distinction between body-cell and germ-cell does not exist. Such organisms are reproduced by division, and if, therefore, any one of them becomes changed in the course of its life by some external influence, and thus receives an individual character, the method of reproduction insures that the acquired peculiarity will be transmitted to its descendants. If, for instance, a Protozoon, by constantly struggling against the influence of mechanical currents in water, were to gain a somewhat denser and more resistant protoplasm, or were to acquire the power of adhering more strongly than other individuals of its species, the peculiarity in question would be directly continued on into its two descendants, for the latter are at first nothing more than the two halves of the former. It therefore follows that every modification which appears in the course of its life, every individual character, however it may have arisen, must necessarily be directly transmitted to the two offspring of a unicellular organism" (p. 277).

"We are thus driven to the conclusion that the ultimate origin of hereditary individual differences lies in the direct action of external influences upon the organism. Hereditary variability cannot, however, arise in this way at every stage of organic development, as biologists have hitherto been inclined to believe. It can only arise in the lowest unicellular organisms; and when once individual difference had been attained by these, it necessarily passed over into the higher organisms when they first appeared. Sexual reproduction coming into existence at the same time, the hereditary differences were increased and multiplied, and arranged in ever-changing combinations" (p. 279).

"It is, however, obvious that sexual reproduction will readily afford such combinations of acquired characters, for by its means the most diverse features are continually united in the same individual, and this seems to me to be one of its most important results.

"I do not know what meaning can be attributed to sexual reproduction other than the creation of hereditary individual characters to form the material upon which natural selection may work" (p. 281).

In the essay entitled "On the Number of Polar Bodies and their Significance in Heredity," Prof. Weismann explains his conception of the mode in which sexual reproduction promotes variability, showing, with the assistance of diagrams, how the nuclear germ-plasm of a fertilized ovum contains germ-plasms derived from the ancestors of both parents.

The conception of the process of variation which the preceding passages (as well as others) produce in the mind of the reader is that unicellular organisms acquired, during the period of their entirely asexual reproduction, a number of individual differences; and that, since the appearance of sexual reproduction, these ancestral characters have been combined in an infinite number of ways, leading to the evolution of all existing varieties of

plants and animals, to say nothing of all the varieties which have perished in the struggle for existence. It would, in fact, appear that Prof. Weismann denies the acquisition of any new individual characters due to the influence of external conditions by any except unicellular organisms.

This being his view, we find, as might be expected, that Prof. Weismann opposes the assumption of the transmission, by means of sexual or amphigonic reproduction, of characters which he terms "somatogenic" (p. 413); that is, of characters which have manifested themselves in the *soma* of an individual, not spontaneously, but as the result of the operation of external forces or conditions; and he critically sifts the evidence for such transmission with results which, it must be admitted, tell in favour of his views.

For all that, Prof. Weismann does not take up an altogether uncompromising position with reference to this point; in fact, his statements of opinion are so fluctuating that it is difficult to determine what his position exactly is: witness the following quotations:—

". . . and it is impossible to imagine any way in which the transmission of changes, produced by the direct action of external forces upon the somatic cells, can be brought about" (p. 80).

"Hence it follows that the transmission of acquired characters is an impossibility . . ." (p. 266).

"For the germ-cells are contained in the organism, and the external influences which affect them are intimately connected with the state of the organism in which they lie hid. If it be well nourished, the germ-cells will have abundant nutriment; and, conversely, if it be weak and sickly, the germ-cells will be arrested in their growth. It is even possible that the effects of these influences may be more specialized; that is to say, they may act only upon certain parts of the germ-cells. But this is indeed very different from believing that the changes of the organism which result from external stimuli can be transmitted to the germ-cells, and will re-develop in the next generation at the same time as that at which they arose in the parent, and in the same part of the organism" (p. 103).

"Still we cannot exclude the possibility of such a transmission occasionally occurring; for, even if the greater part of the effects must be attributed to natural selection, there might be a small part in certain cases which depends on this exceptional factor.

"A complete and satisfactory refutation of such an opinion cannot be brought forward at present; we can only point out that such an assumption introduces new and entirely obscure forces, and that innumerable cases exist in which we can certainly exclude all assistance from the transmission of acquired characters" (p. 100).

"If, on the other hand, acquired differences are transmitted, this would prove that there must be something wrong in the theory of the continuity of the germ-plasm, as above described, and in the non-transmission of acquired characters which results from this theory" (p. 268).

"It seems to me that the problem of the transmission or non-transmission of acquired characters remains, whether the theory of the continuity of the germ-plasm be accepted or rejected" (p. 403).

I would remark, with reference to the statement that it is impossible to imagine any way in which somatogenic changes can be transmitted, that such a transmission is quite conceivable, and is even probable, when the continuity of the somatoplasm is borne in mind. If the ovum contains somatoplasm, as we are driven to assume, and if, as cannot be denied, the somatoplasm takes part in the formation of the body of the embryo, then it is not impossible that changes induced in the body of the parent, by the action of external conditions, may be transmitted to the offspring through the somatoplasm of the ovum. The discontinuity of the somatoplasm must be proved before the impossibility of the transmission of somatogenic characters can be considered to have been established.

But if Prof. Weismann is not prepared to admit that there is more than a remote possibility that variation may, in some degree, be due to the transmission of somatogenic characters, he makes a large concession in admitting that new characters may be acquired in another way, and, being transmissible, lead to variation. The first hint of this view is to be found on pp. 98, 99:—

"These changes—such, for example, as are produced by a strange climate—have always been looked at under the supposition that they are transmitted and intensified from generation to generation, and for this reason the observations are not always sufficiently precise. It is difficult to say whether the changed climate may not first have changed the germ, and if this is the case the accumulation of effects through the action of heredity would present no difficulty" (p. 98).

"It must be admitted that there are cases, such as the climatic varieties of certain butterflies, which raise some difficulties against this explanation. I myself, some years ago, experimentally investigated one such case, and even now I cannot explain the facts otherwise than by supposing the passive acquisition of characters produced by the direct influence of climate" (p. 99; see also above quotation from p. 103).

It is again mentioned on p. 271, but it is not prominently asserted until p. 410, where Prof. Weismann says:—

"I have never doubted about the transmission of changes which depend upon an alteration in the germ-plasm of the reproductive cells, for I have always asserted that these changes, and these alone, must be transmitted. If anyone makes the contrary assertion, he merely proves that he does not understand what I have said upon the subject. In what other way could the transformation of species be produced, if changes in the germ-plasm cannot be transmitted? And how could the germ-plasm be changed except by the operation of external influences, using the words in their widest sense? . . ."

On pp. 402-403 Prof. Weismann defines his view more clearly:—

"It is certainly necessary to have two terms which distinguish between two chief groups of characters—the primary characters which first appear in the body itself, and the secondary ones which owe their appearance to variations in the germ, however such variations may have arisen. We have hitherto been accustomed to call the former 'acquired characters,' but we might also call them *somatogenic*, because they follow from the reaction of the *soma* under external influences; while all other characters might be contrasted as *blastogenic*, because they include all those characters in the body which have arisen from changes in the germ. In this way we might perhaps prevent the possibility of misunderstanding. . . . Among the blastogenic characters, we include not only all the changes produced by natural selection operating upon variations in the germ, but all other characters which result from this latter cause."

The point is again mentioned on p. 433:—

"It is therefore possible to imagine that the modifying effects of external influences upon the germ-plasm may be gradual and may increase in the course of generations, so that visible changes in the body (*soma*) are not produced until the effects have reached a certain intensity."

It is not a little remarkable that, after insisting so strongly, as in the passage previously quoted, on the extreme stability of the germ-plasm, Prof. Weismann should be prepared to admit that it is in so high a degree susceptible to the action of external influences. He is, however, inclined to complain, in the passage on p. 410, that this view of the production of blastogenic changes by external influences has been ignored; but the readers of the earlier essays may well be pardoned for inattention to this point, as it is only casually mentioned there, and is not put forward as an integral part of his theory of variation. No one reading the statement of his theory of variation on p. 277 would infer that Prof. Weismann attached any importance to the effect of external influences on the germ in producing new characters. In fact,

Prof. Weismann himself seems hardly to realize how inevitable such a conclusion is. If it be admitted that unicellular organisms acquire new characters under the operation of external influences, it cannot consistently be doubted that this also takes place in germ-cells.

It seems to be necessary, therefore, to modify the conception of variation founded upon the above-quoted paragraph from p. 277, by introducing into it the operation of external influences upon the germ. We now see that though sexual reproduction greatly promotes variation in consequence of the ever-new combination of ancestral characters in each fertilization, yet another efficient cause of variation is the direct action of external influences on the germ, giving rise to blastogenic characters.

But this modified conception of the causes of variation comes into collision with Prof. Weismann's statement (p. 277) that "the origin of hereditary individual variability cannot indeed be found in the higher organisms—the Metazoa and Metaphyta; but it is to be sought for in the lowest—the unicellular organisms," a collision which is much to the detriment of the latter; for, if it cannot be denied that external influences give rise to blastogenic characters, then it cannot be maintained that "the origin of hereditary individual variability cannot be found in the higher organisms." On the contrary, it must be admitted that the modifying influence of external conditions continually affects not only unicellular organisms, but also the germ-cells of the Metazoa, producing new characters, thus inducing variation, in both.

This conclusion leads to the consideration of a point of great interest. In accordance with his view of the preeminent importance of amphigonic reproduction in causing variation, Prof. Weismann asserts the lack of variability in parthenogenetic forms, in the following words (p. 290):—

"If my theory as to the causes of hereditary individual variability be correct, it follows that all species with purely parthenogenetic reproduction are sure to die out; not, indeed, because of any failure in meeting the existing conditions of life, but because they are incapable of transforming themselves into new species, or, in fact, of adapting themselves to any new conditions. Such species can no longer be subject to the process of natural selection, because, with the disappearance of sexual reproduction, they have also lost the power of combining and increasing those hereditary individual characters which they possess."

The views contained in this paragraph appear to me to be completely at variance with the facts known concerning the Fungi, among plants. Thus, in the Saprolegnieæ, all the known forms, including several genera and many species, are parthenogenetic; the sexuality of the Ascomycetes is still the subject of discussion, but it is admitted that many genera and species of these Fungi are certainly asexual; and the sexuality of the Æcidiomycetes is extremely doubtful. These plants show no apparent tendency to die out, in spite of the absence of sexuality. But it may be replied that these families may be in the stage in which sexuality is just disappearing, and in which they are still adequately adapted to their conditions of life. Such an objection cannot apply, however, to the Basidiomycetes. These Fungi are not only entirely asexual, but it would appear that they have been evolved in a purely asexual manner from asexual ascomycetous or Æcidiomycetous ancestors. The Basidiomycetes, in fact, afford an example of a vast family of plants, of the most varied form and habit, including hundreds of genera and species, in which, so far as minute and long-continued investigation has shown, there is not, and probably never has been, any trace of a sexual process. How are we, then, to account for all the variation which has taken place in this group, quite independently of amphigonic reproduction? On this point Prof. Weismann says (p. 275):—

"If it could be shown that a purely parthenogenetic species had become transformed into a new one, such an observation would prove the existence of some force of transformation other than selective processes, for the new species could not have been produced by these latter."

It appears to me beyond doubt that, in the Fungi, new species have been developed from parthenogenetic forms, but I leave it to Prof. Weismann to suggest what "force of transformation other than selective processes" may have been operative.

It is not, however, argued that the variation of the higher Fungi is as great as it might have been had they possessed sexuality; for there can be no doubt that sexual reproduction does very materially promote variation. It seems probable, in fact, that the absence of sexuality in these plants may be just the reason why no higher forms have been evolved from them; for in this respect they present a striking contrast to the higher Algæ in which sexuality is well marked.

Since it is clear that new hereditary characters can be produced by the action of external influences on the germ, the outcome of Prof. Weismann's investigation of the phenomena of variation is that he has given prominence to the fact that new hereditary characters need not be apparent in the body of the parent, but that, on the contrary, the somatogenic characters are just those which are least likely to be transmitted. This is essentially the same position, though stated in more precise terms, as that taken up by Darwin, who held that it is not the sudden variations, due to altered external conditions, which become permanent, but those slowly produced by what he termed the accumulative action of changed conditions of life.

With this I close my criticisms, not because there are no other points which might be discussed, but because I have already touched upon many of them in my "Lectures on the Physiology of Plants" (Cambridge, 1886), and because I desire at present to deal solely with the more fundamental parts of the theory. I have, I think, said enough to show that, interesting and suggestive as is Prof. Weismann's theory of the continuity of the germ-plasm, it by no means affords, at least in its present form, so complete and ready an explanation of the facts of embryogeny, heredity, and variation, as the enthusiasm of some of his more ardent disciples would have us believe. SYDNEY H. VINES.

Oxford, September.

NOTES.

WE regret to announce the death of Mr. John Ball, F.R.S., which took place somewhat suddenly at midnight on Monday last. We understand that the funeral will take place to-morrow (Friday), at 11 a.m., at St. Thomas's, Walham Green.

THE Reports of the Eclipse Expedition of 1886 are at length ready for publication, and will be issued immediately as separate numbers of the Philosophical Transactions. The first, "On the Total Solar Eclipse of August 29, 1886," is by Captain Darwin, Dr. Schuster, and Mr. Maunder; the second, "On the Observations made at the Island of Carriacou," is by the Rev. S. J. Perry; the third, "On the Determination of the Photometric Intensity of the Coronal Light," by Captain Abney and Prof. Thorpe; and the fourth, "On the Observations made at Grenville, in the Island of Grenada," by Mr. H. H. Turner.

THE collection of objects brought back by Prof. Haddon from various islands in Torres Straits is now to be seen in a part of the Eastern Assyrian room at the British Museum, on the upper floor of the north-east angle of the building. Special interest attaches to the anthropological specimens included in this valuable collection.

THE list of names to be recommended for the new Council of the London Mathematical Society at its annual meeting, on November 14, differs from that of last year in the following

respect: the names of Prof. Cayley, F.R.S., and of Prof. W. Burnside will be submitted to fill up the vacancies caused by the retirement of Dr. E. J. Routh, F.R.S., and Prof. Hart, of Woolwich.

THE Library of the Royal College of Surgeons will, as an experiment, be, for the remaining portion of the present year, open in the evening. On each week-day except Saturday the hours will be from 11 a.m. to 9 p.m.; on Saturdays the Library will be closed at 1 p.m.

A PHYSICAL SOCIETY is to be formed in Liverpool. It will hold its meetings at University College, and Prof. Oliver Lodge has consented to be nominated as first President. The preliminary meeting will be held in the Physics Theatre of University College, at 8 o'clock on Wednesday evening, November 6, Prof. Oliver Lodge in the chair. The Secretary (*pro tem.*) is Mr. Thomas Tarleton, 1 Hyde Road, Waterloo, Liverpool.

MR. JOHN W. MCCOY, who died in Baltimore lately, bequeathed his library, with 100,000 dollars, to the Johns Hopkins University.

ON Monday a statue of J. B. Dumas, the chemist, was unveiled at Alais, his native town, by M. Faye, French Minister of Agriculture.

LIKE many other schools, Fettes College, Edinburgh, has long been provided with chemical laboratories, but has had no room specially adapted for the teaching of physical science. This want has been met by the construction during the summer of a well-appointed physical laboratory fitted up both for lectures and for practical work by boys themselves. There is a well-arranged supply of gas and water, several stone slabs for delicate instruments affected by oscillations, screens for projection experiments, a heliostat window and system of blinds for darkening the room for experiments on light. A smaller room leading off from the main room is reserved for special work, and is also fitted up as a dark room for photography. The laboratory is close to the boys' workshop, and to the workshop of the resident carpenter and mechanic.

AN interesting Report by Sir Brandford Griffith, Governor of the Gold Coast, has lately been issued by the Colonial Office. It describes at considerable length a tour through the interior of the colony, undertaken for the purpose of investigating the gold deposits and examining the gold-mines at work. The Governor comes to the conclusion that the country is rich in gold, and that it is merely a matter of the necessary time and scientific application for the metal to pay well for extraction. He finds, also, that "earnest and well-considered attempts" are now being made to secure success. An appendix containing a special Report on the Winnebah district and its mineral wealth is added.

AT the first meeting of the new session of the Geological Society of Glasgow, Mr. John Young exhibited a fine series of specimens of Polyzoa and Monticuligrora, comprising over twenty species, which he had this summer discovered at Kirktonhelm, East Kilbride. He remarked that the shale which holds these organisms lies between the first and second Calderside limestones. Most of the specimens were taken from blocks of shale which had been partially burnt in the lime-kilns, the rock in its natural condition being of a gray colour, but changed by contact with heat into yellowish gray. From this change of colour the fronds of the Polyzoa, with their lace-like structure, stand out much more prominently than when the shale is in its original state. They have also the advantage of being very much hardened, whereas the shale is naturally very friable, decomposing almost whenever it is exposed to the action of the weather. Mr. James Bennie, of the Geological Survey of Scotland, read a paper, "Things New and Old from the Ancient Lake of Cowdenglen." Up to 1867 the picturesque little valley of Cowden, beyond Crofthead, on the road to Ayrshire, was not known to possess any features of special geological interest, but in that year, having been chosen as the route of the direct railway to Kilmarnock, it was invaded by the navvy with pick and shovel, to the utter destruction of all its natural beauties. The gradients being steep, the excavations were extensive, and at one point the bed of an ancient lake was cut through, containing deposits of mud and peat lying between two distinct layers of boulder clay. These stratified deposits were found to contain numerous remains of animal and vegetable life, both of higher and lower forms, and, as their presence afforded strong evidence of the great ice age, a controversy arose between the upholders of a single glacial period and those who believed in two or more. Of late years there has been a revival of interest in old lake deposits, which led the author to re-examine the subject and the specimens of the deposits in his possession. Always a supporter of the "interglacial" or successive theory of ice periods, he re-stated his case in the present paper, bringing forward a fresh accumulation of proof.

MESSRS. KING, MENDHAM, AND CO., of the Western Electrical Works, Bristol, have sent us a sample of a very convenient pocket galvanometer which they are now making. It is about the size of an ordinary watch, the ring suspender acting also as a binding screw. It is not only a detector galvanometer, but can be used for the absolute measure of currents up to one ampere. The dial is accordingly graduated one half in degrees. and the other in milliamperes, and is so made that either half can be brought to the top, whilst the needle remains in a vertical position. The front portion carries the dial and magnetic needle, and the back part of the case contains the bobbins wound with copper wire, the resistances of which have been measured and particulars furnished with the instrument. Small stands are also supplied for laboratory use if required. The instrument is beautifully finished, and we strongly recommend it to all who use batteries in any shape or form. To those who use electricity for medicinal purposes, where the current is of prescribed strength, such an instrument must be indispensable, and its importance to electric-bell fixers and telephone and telegraph worker is obvious. To economical users of batteries it will recommend itself, as many a good cell may be saved by the detection of the one which is really at fault. Full instructions are supplied with each instrument.

A GOOD word has at last been said for the sparrow in America. In England this impudent bird is decidedly popular, but our kinsfolk in the United States consider him an unmitigated nuisance. Captain W. F. Segrave, British Consul at Baltimore, writing on the subject in a recent Report, warns the Americans that their policy in waging war against the sparrow may prove to be a mistake. "The great 'blizzard' of March 1888," he says, "destroyed multitudes of sparrows, and as a consequence, the past and present summers have seen a vast increase in grubs and caterpillars. Already in many large cities the inhabitants through the public press are complaining of the destruction of their ornamental trees, the diminished number of sparrows being unable to keep in check the vast increase which has taken place in noxious grubs, worms, and caterpillars."

THE third and concluding portion of Prof. Kikuchi's (of Tokio) work on geometry has just reached us. It is entitled "Rittaikikagaku," or "Solid Geometry." He has not been able to derive assistance in its compilation from the work of the Association for the Improvement of Geometrical Teaching, as the syllabus of this subject is yet in an embryo state. If we may judge from the figures, he has adapted portions of Euclid xi., 1–21, and of Wilson's "Solid Geometry," with a brief treatment of the regular solids and the elements of spherical geometry. There is an appendix of the equivalents of English terms written in Japanese and Roman letters.

MESSRS. GURNEY AND JACKSON have in the press "A Handbook of European Birds," by James Backhouse, Jun. The author explains that, having frequently experienced the need of a handy reference volume on European birds, he has been at great pains to meet this want, by endeavouring to produce a complete series of short general descriptions, in a convenient form either for the portmanteau or the pocket. The work will be published by subscription during the autumn or early spring.

THE last published number (31) of *Excursions et Reconnaissances*, the official scientific and learned journal of the French possessions in Indo-China, contains the first of a series of articles, by M. Aymonier, the well-known scholar, on the writing, dialects, history, manners, and customs of the Chams, the ancient masters of Annam proper. The present instalment deals with the grammar, and is illustrated by some lithographs of their writing and their curious cursive alphabets. These are followed by a portion of a Romanized version, with a translation, in French prose, of an Annamite rhymed tragedy.

IN our abstract of the chemical papers at the British Association (p. 587) we gave an account of the paper on alloys read by Messrs. Heycock and Neville. These gentlemen send us the following expansion of our statement:—"(1) Our experiments lead us to the conclusion that the *molecule of aluminium when in solution in tin is* $Al_2 = 54$, and not, as stated, that its atomic weight is different from the accepted value. (2) We stated that the mere application of Raoult's method to alloys does not decide the molecular weight of metals in solution, because we have no standard molecule, of which the molecular complexity in solution is certainly known, to take as unity. By applying Van 't Hoff's theory of solution to alloys of tin, we calculate a number for the fall in the freezing-point produced by one *molecule* of metal in 100 molecules of tin which is almost identical with the constant fall found in our experiments for *one atom* of metal in 100 of tin. Hence we conclude that, with the exception of aluminium, and possibly indium, all the seventeen metals we have examined have single atom molecules when in solution in tin. Our experiments, therefore, in the main, confirm Prof. Ramsay's results obtained by another method, and E. Tamman's results with amalgam."

METHYL HYDRAZINE, $CH_3-NH-NH_2$, the simplest derivative of hydrazine or amidogen, $\begin{matrix} NH_2 \\ | \\ NH_2 \end{matrix}$, has been isolated by Dr. Gustav von Brüning in the Chemical Laboratory of the University of Wurzburg (*Liebig's Annalen*). It is a clear and very mobile liquid, boiling at 87° C., and possessing a most violent affinity for water, resembling in this respect the recently-isolated hydrazine itself. Its odour is very similar to that of methylamine, and the vapour produces a white cloud in the air due to absorption of moisture to form minute drops of the liquid hydrate. Brought in contact with water, it instantly dissolves with evolution of great heat. It reduces Fehling's solution in the cold, and decomposes nitrous acid with copious evolution of free nitrogen. Its hygroscopic character is so pronounced that it attacks the skin in a most painful manner, and rapidly destroys corks or caoutchouc stoppers. The mode of preparation finally adopted by Dr. von Brüning is in reality very simple, the only difficult operations being those involving its separation from water. It consists in first converting the mono-methyl derivative of urea, $CH_3-NH-CO-NH_2$, into the nitroso-compound, $CH_3-N(NO)-CO-NH_2$; this, upon reduction with zinc dust and glacial acetic acid, yields the corresponding hydrazine urea, which in turn is broken up by boiling with hydrochloric acid into carbonic anhydride, ammonia, and methyl hydrazine. In practice, 50 grams of nitrate of methyl urea are dissolved in warm water, and the solution cooled until crystals begin to form. Crushed ice is then added to keep the temperature as low as possible during the addition of the calculated quantity of solid sodium nitrite; the moment the nitrite is added, the nitroso-compound commences to separate in small yellow lamellæ, and may be obtained recrystallized from ether in much larger slightly yellow tables. The reduction of this nitroso body is then effected by suspending it in water and adding glacial acetic acid and zinc dust, the latter in small portions at a time, with constant agitation. The cold solution, filtered from excess of zinc dust, is then saturated with hydrochloric acid, and afterwards evaporated to a syrupy consistency. The syrup is next boiled for some hours with a concentrated solution of hydrochloric acid in a flask connected with an inverted condenser, after which the liquid is neutralized with a strong soda solution at a low temperature, and sufficient excess of soda added to redissolve the precipitated zinc hydrate. The alkaline solution is then distilled in steam, when the base rapidly and completely passes over, ammonia and methylamine escaping as gases. After removal of most of the dissolved ammonia and methylamine by boiling in the flask supplied with inverted condenser, the base is converted to its acid sulphate, $CH_3-NH-NH_2 . H_2SO_4$, and the crystals of this salt are distilled with a concentrated solution of soda containing pieces of solid sodium hydrate. The last trace of ammonia escapes, while the methyl hydrazine condenses in the cooled receiver. After allowing the distillate to remain in contact with solid soda for twenty-four hours, and then re-subjecting it to distillation, it is still found to contain water. It was, however, finally freed from water by heating to 100° C. in a sealed tube with anhydrous barium oxide.

THE additions to the Zoological Society's Gardens during the past week include a Pigtailed Monkey (*Macacus nemestrinus* ♂) from Java, presented by Mrs. Cosh; three Common Hedgehogs (*Erinaceus europæus*), British, presented by Mr. H. Pelham Curtis; two Cayenne Aracaris (*Pteroglossus aracari*) from Macey, Brazil, presented by Mr. Thomas Watson Permain; a Red and Blue Macaw (*Ara macao*) from Central America, presented by Mr. Robert Romer, Q.C.; a —— Hawk (*Asturina* sp. inc.) from Brazil, presented by Mr. J. E. Wolfe; a Well-marked Tortoise (*Homopus signatus*), a Rufescent Snake (*Leptodira rufescens*), three Smooth-bellied Snakes (*Homalosoma lutrix*), a Many-spotted Snake (*Coronella multimaculata*), a Cape Adder (*Vipera atropos*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; two Macaque Monkeys (*Macacus cynomolgus* ♂ ♀) from India, deposited; six Indian Pythons (*Python molurus*), an Indian Cobra (*Naia tripudians*) from India, purchased.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *OCTOBER* 27—*NOVEMBER* 2.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 27

Sun rises, 6h. 48m.; souths, 11h. 43m. 55·9s.; daily decrease of southing, 4·9s.; sets, 16h. 40m.: right asc. on meridian, 14h. 8·0m.; decl. 12° 57′ S. Sidereal Time at Sunset, 19h. 5m.

Moon (at First Quarter October 31, 9h.) rises, 10h. 12m.; souths, 14h. 29m.; sets, 18h. 39m.: right asc. on meridian, 16h. 54·0m.; decl. 20° 49′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	5 2 ...	10 40 ...	16 18 ...	13 4·4 ...	4 56 S.
Venus.....	4 6 ...	10 4 ...	16 2 ...	12 28·0 ...	1 16 S.
Mars......	2 38 ...	9 6 ...	15 34 ...	11 29·5 ...	4 46 N.
Jupiter....	12 0 ...	15 53 ...	19 46 ...	18 18·3 ...	23 29 S.
Saturn....	0 48 ...	7 54 ...	15 0 ...	10 17·4 ...	12 3 N.
Uranus ...	5 41 ...	11 2 ...	16 23 ...	13 26·4 ...	8 28 S.
Neptune..	17 58*...	1 47 ...	9 36 ..	4 9·1 ...	19 17 N.

* Indicates that the rising is that of the preceding evening.

Oct.	h.	
28	23	Jupiter in conjunction with and 0° 7′ south of the Moon.
31	16	Mercury at greatest elongation from the Sun, 19° west.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·5	81 17 N.	Oct. 30,	1 44	*m*
Algol	3 1·0	40 32 N.	,, 27,	2 15	*m*
			,, 29,	23 4	*m*
U Monocerotis	7 25·5	9 33 S.	,, 31,		*m*
R Cancri	8 10·4	12 4 N.	,, 30,		*M*
U Ophiuchi	17 10·9	1 20 N.	,, 30,	21 36	*m*
X Ophiuchi	18 33·1	8 44 N.	,, 30,		*M*
β Lyræ	18 46·0	33 14 N.	,, 29,	20 30	*M*
			Nov. 2,	2 0	*m*
U Aquilæ	19 23·4	7 16 S.	,, 2,	20 0	*M*
η Aquilæ	19 46·8	0 43 N.	,, 2,	1 0	*m*
S Sagittæ	19 51·0	16 20 N.	Oct. 29,	23 0	*M*
T Vulpeculæ	20 46·8	27 1 N.	Nov. 1,	0 0	*m*
			,, 2,	2 0	*M*
δ Cephei	22 25·1	57 51 N.	,, 1,	23 0	*m*

M signifies maximum: *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ε Arietis	43°	22° N.	Slow; brilliant.
	55	9 N.	,, ,,
,, χ Cancri	105	12 N.	Swift; streaks.
	132	22 N.	Very swift.

THE GEOGRAPHICAL PAPERS AT THE BRITISH ASSOCIATION.

SCIENTIFIC geography did not form a prominent feature in the Geographical Section at Newcastle. As was right and proper in so important an industrial centre, it was evidently intended to devote special attention to commercial geography. The success was only partial. It will have been seen that the President, Sir Francis De Winton, devoted a considerable part of his address to pointing out some of the important practical applications which may be made of geographical knowledge. Again, one of the ablest and most instructive papers read in the Section was by Dr. Hugh Robert Mill, on the Physical Basis of Commercial Geography. A *necessary preliminary*, Dr. Mill pointed out, to the study of commercial geography is a full acquaintance with topography, especially with the names and positions of all commercial towns. A *necessary accompaniment* to the study of commercial geography is a knowledge of the ever-varying relations between regions of supply and demand, the incidence of tariffs, and the political and social conditions of countries. The *physical basis* of commercial geography, which underlies and gives unity to the whole subject, is a knowledge of the resources of the earth as regards the various existing forms of matter and modes of energy, the best means of separating, combining, and modifying these so as to produce commodities, and the way in which commodities can be best transported. Commerce being the artificial redistribution of the matter and energy of the world, a knowledge of the general properties and the unchangeable laws of matter and energy should take a chief place in the training of commercial men. A general acquaintance with this practical science, which may be termed *applied physiography*, or *practical earth knowledge*, ought to be possessed by all merchants, and a special branch should be familiar to each. Amongst the advantages which would thus be gained are:—(1) The merchant would understand the principles of the production and manufacture of his goods. (2) He would know in many cases, without aimless and extravagant experiments, where it is possible to produce any special commodity in great abundance. (3) He could, to a great extent, anticipate the frequent changes in staple commodities by knowing what other commodities it is possible to produce in the regions now yielding the staple only. (4) He would understand the best and shortest routes between trade centres. Illustrations and arguments showing the importance of these statements were given in Dr. Mill's paper, and a large map of the commercial development of the world was shown. Dr. Mill has thus done something to give definite shape to a conception of commercial geography. The fact is, applied geography in general, like applied chemistry or applied physics, implies a sound knowledge of the subject as a science. If the facts and principles of the subject are thoroughly known, their application need not be difficult. This application cannot be said to have been very successful in Section E. The evident object in view was to exemplify by special examples the principles laid down in the President's address and in Dr. Mill's paper. Thus we had a series of papers on what purported to be the commercial geography of a number of countries or regions. The geography, however, in most cases was conspicuous by its absence. The papers were certainly most useful in their way, and doubtless would be of some commercial value. Thus Colonel Mark Bell's paper on the great Central Asian trade route from Peking to Kulja and Semrechensk, and to Yarkand and India, abounded with original information collected by an acute observer, and it is hoped will be published in full by the Royal Geographical Society. But the minute details dwelt upon by the author were quite unsuited to an audience. Mr. R. S. Gundry's review of industrial and commercial progress in China was admirable in its way, and the views enunciated by the author original and suggestive. The conclusion come to was that a more widespread desire for progress and radical financial reform will be required before China is likely to rival Japan in the completeness of its transformation.

There was as usual a considerable number of African papers, some of them really good even from the geographical standpoint. Governor Moloney gave much useful information on the Yoruba country and its various tribes, his paper, however, being mainly occupied with suggestions as to its industrial development. The same may be said of Captain Lugard's paper on Nyassaland, and Mr. Rankin's on the Zambesi. The Rev. R. P. Ashe's paper on Buganda contained little not already published in his recent work; it dealt mainly with the natives, their political organizations, their religion, manners, and customs. Captain E. C. Hore's paper on Lake Tanganyika was one of the best in the Section. The author, who has lived ten years on the lake, described its geographical position, as occupying the central depression of the heights of Africa, from the surrounding barrier of which descend the furthest sources of the great rivers; referred to its outlet, the Lukuga, and remarked upon certain earthquake phenomena, and the aspect of the depression and of the bed of the lake. He gave a general description of the lake, with the results of meteorological observations and notices of scenery, and aspects of the lake under various changes of weather. He described the natives living on the shores of the lake and within the central depression, as representing all the great African families, and gave some account of their arts and industries, and of the produce of the lake region. He sketched the African routes and lines of communication as converging towards or crossing the lake, and the present available approaches to the lake from the east coast. He then referred to the position of the lake amongst and in relation to present claims and operations in Central Africa, pointing out what European enterprise has already achieved on the lake.

An excellent paper in physical geography was that of Mr. Flinders Petrie on Wind-Action in Egypt, the results of his own recent observations in the Nile Delta. He stated that the underlying motions of the Delta are depression on the coast and upheaval at Ismailiyeh. Above these movements great changes have been made by wind-action; in some sites at least 8 feet of ground has been removed and deposited in the water. This has partly caused the great retreat of the Red Sea head, and tends to form the characteristic swamps of this district. Formerly the Delta was a desert tract, with valleys inundated by the Nile. Before historic times the Nile valley was deep in water, partly estuarine, partly fluvial, and great rainfall then took place. That this was in the human age is shown by the position of worked flints.

Mr. Batalha Reis, in his paper on recent Portuguese explorations in Africa, put in a claim for exploring activity on behalf of Portugal which it would be difficult to substantiate. Mr. E. G. Ravenstein made some important corrections in the course of the Upper Nile as laid down in recent German maps.

Mr. Basil Thomson's paper, on his recent expedition to the D'Entrecasteaux and Louisiade Islands, was the same as that given some time ago to the Royal Geographical Society, and reported in NATURE. Dr. Carl Lumholtz's paper, on the present and future of Queensland, was highly interesting and useful from a colonial point of view. He, moreover, gave some of the

results of his own observations during the time he lived with the natives as one of themselves. He found them to be undoubted cannibals, and predicts their early extinction.

One of the most original and scientific papers in the Section was that of Dr. Guppy on the south coast of West Java. The author dealt with a part of Java which has not been much described. It is one of the least familiar portions of this large island, a circumstance due partly to its paucity of anchorages and to the difficulty in landing; partly to its having been allowed to become in some places a kind of menagerie; and partly, also, to the fact that it lies remote from the chief seats of government. Now that the Netherlands Indian Government are rapidly carrying out their systematic survey of the Preanger Residency, it will not be long before the south coast of West Java will be much better known than it is at present; and the recent extension of the central railway to Garoet and Tjirajap will do much to effect this end. The author's tracks over West Java would make a chequered pattern on a map; but he has thought it best not to refer to localities already well known—localities which are now yearly visited by hundreds of visitors. Taking the central railway as his base, he performed nearly all the distance on foot, walking about 560 miles in all. In the paper he endeavoured to give a general idea of this south coast alone. The huge volcanic cones were landmarks to him, and nothing more; they had been well described by Junker and others, so he resisted the temptation of climbing them, and reserved his main efforts for the examination of the little-described and remote south coasts of the Preanger and Bantam Residencies. The object he had in view was to ascertain what physical evidence there was for the belief that the west end of Java was originally united with Sumatra. In this paper the author showed that all the evidence on the Java side of the Sunda Strait points to the opposite conclusion. Zoological evidence cannot be held sufficient to establish the previous connection between two islands without the physical evidence of such a change The problem, as usually stated, seems to begin at the wrong end of the matter. Given the present distribution of plants and animals, it is then attempted to explain the previous arrangement of the land, and this is done too often without appealing to the physical evidence at all. In tracing geographical changes in the past, it would seem more reasonable to adopt an opposite method; but in the great majority of cases affecting the distribution of animals, it would be wiser in the first place to assume the *status quo*, and fall back when that fails on the physical evidence of the presumed changes.

As was rightly pointed out by Mr. H. J. Mackinder, Dr. Guppy's apparent contempt of the argument from zoological distribution is to be deplored. Hitherto it has been regarded, and rightly so, by the ablest biologists and geographers, as one of the surest and most valuable keys to past geographical conditions; and it will require much more powerful arguments than Dr. Guppy was able to adduce in his paper to cast it aside.

In a paper on recent explorations in Peru and Bolivia, Mr. H. Guillaume described the efforts which have been made by Peruvian and Bolivian explorers and traders to open up the rivers and the dense forest country lying between them. Colonel Labre since 1872 has been endeavouring to open communication from the Purus to the Beni. He explored the River Itury and its affluents several times, as to the character and navigability of which he has contributed much new information. Padre Nicolas Armentia explored the Madre de Dios in 1885, and resided for some time in the country of the Araonas Indians. From its mouth for 280 miles the river receives no important tributary: the Padre believes it has a navigable course of 400 miles for steamers. Mr. Guillaume described in detail the gold-bearing region at the source of the Madre de Dios. He then referred to the explorations of Senor Carlos Fry on the Ucayli and its tributary, the Urubamba.

Mr. Theodore Bent's paper, on his recent visit to the Bahrein Islands in the Persian Gulf, was a contribution of some originality on the present condition, the antiquity, the inhabitants, and past history of this interesting group. Dr. Nansen's paper on Greenland was identical with that given to the Geographical Society, and already reported in NATURE.

The Report presented to this Section by Mr. Joseph Thomson, on the geography and geology of the Atlas Mountains, can hardly be said to contain anything that has not already appeared in his narrative, except the lists of plants and of Coleoptera.

On the whole, it will be seen that the Geographical Section has not a very brilliant account to render.

THE MECHANICAL PAPERS AT THE BRITISH ASSOCIATION.

AMONG the papers read in Section G, after the President had delivered his address, was one by Mr. Alex. C. Humphreys, on water-gas in the United States. Water-gas is produced by the decomposition of steam by incandescent carbon. The two ways of effecting the decomposition, the intermittent and continuous, were described. In the first a cupola furnace is used: a blast of air raises the fuel to the necessary temperature; when this is effected the air is cut off and steam turned on, the blowing in of air and steam occurring intermittently. In the continuous process, steam is passed uninterruptedly through retorts containing carbon, which are heated externally, or steam and air are forced in continuously through a cupola furnace; but the latter process has the disadvantage of the resultant gases containing nitrogen. Water-gas has no light-giving properties so that it has to be carburized for illuminating purposes, or employed to raise some solid substance to a white heat. The different processes in vogue were described, and their theory explained. In conclusion the author gave expression to the belief that the day of gas, fuel-gas, was rapidly approaching; that even the great rival of gas, the electric light, may yet be dependent on it for the cheapest means of producing the electric current. Then will the gas engineer and the electrical engineer, shoulder to shoulder, be striving to correct the present wasteful strains on Nature's storehouses.

Precautions to be adopted when the electric light is supplied by means of transformers, by Mr. Killingworth Hedges. In a paper the author read at the Southport meeting of the British Association, he urged the necessity of regulations, and the adoption of proper safety appliances, in connection with electric lighting. In this paper he refers to the danger incurred when currents of high tension are converted into pressures suitable for incandescent lamps by means of transformers. The precaution necessary in such cases is either to earth the secondary circuit—which, however, has certain disadvantages—or to connect one or both of the leads to a safety appliance, which would automatically divert any excess current to earth, and at the same time shut off the supply in that portion of the faulty circuit by the fusion of the lead wire or mica-foils in the main cut-outs. Numerous experiments have been made with a vacuum protector, designed by the author, to ascertain the distance which an alternating current of high E.M.F. will leap across the two electrodes, which were fixed in the opposite ends of a glass tube from which the air had been partially excluded. The results differ from those observed by De la Rue with continuous currents; the following phenomenon was noticed—that the arc, after starting between the two points, almost invariably extended itself to a bow-shape and ran back to the base of one or both of the platinum electrodes, one of which nearly always fused, leaving the other intact.

Electric launches on the Thames, by Prof. G. Forbes, F.R.S. Launches are chiefly wanted in the summer; to prevent injuries to banks the speeds should not be high, so that a comparatively small supply of accumulators is required. The author experimented with the *Delta*, 33 feet long, 6 feet beam, fitted with forty-four cells, weighing 2520 pounds. She is steered by a wheel in front within reach of three handles required to work her. The first is to put the current on or off; the second for half or full speed, and the third for going ahead or astern. The first is mechanically locked with the others, so that they cannot be moved without first cutting off the current. Fusible cut-outs are inserted to prevent injury to the motor if the propeller becomes jammed. The batteries are under the seats on each side of the boat, thus leaving clear space for passengers, of which she could carry twenty. The pull at full speed gives 1·44 horse-power, or 1074 watts, including electrical losses, slip, and all friction. The average pressure at the motor terminals during the run was 78 volts, and the average current 23 amperes, which gives 1794 watts expended. This gives a total efficiency of 60 per cent. The author suggested that negotiations should be opened with the Thames Conservancy to establish charging stations, as there was likely to be a great demand in the future for electric launches.

Series electrical traction (Northfleet Tramways), by Mr. Edward Manville, M.Inst.C.E. The economical distribution of electrical power over long distances involves the use of a current of high potential, and by running the motors in series the advantages of high potential are secured. The main features of a series electrical tramway are a dynamo producing a current

of constant quantity, a closed metallic circuit of which travelling motors at all times form a part without ever being short-circuited, or having the current supply cut off from them, and the regulation of the power developed by the motor and absorbed by it without interrupting the continuity of the circuit. An insulated cable connected to one terminal of the generator traverses the whole length of the line, and is interrupted at distances of 20 feet, the divided ends being connected with the opposite faces of a "spring jack," which is at the same time the automatic switch and contact point. The current collector, which is the same length as and carried by the car, is constructed so as to pass between the faces of the "spring jack," and conduct the current to the motor without at any time short-circuiting it or interrupting the main circuit. In series running, there is little danger of damage to the motor by careless driving, or reversing while running; in descending a gradient there is positive advantage in checking the speed of the car by altering the field connections, so that the armature tends to revolve in the opposite direction to that in which the car is travelling; the power that would otherwise be lost in braking is thus added to that produced by the generator.

Telephonic communication between London and Paris, by Mr. W. H. Preece, F.R.S. The difficulty of such a communication was not the distance, 275 miles, between the two towns, as in the United States speech is maintained between New York and Boston, 350 miles apart; but the insertion of underground wires at each end, and of a cable in the middle, places difficulties in the way that have to be surmounted. The author has experimented on the cables between Dover and Calais and others, and finds the conditions to be fulfilled simple. The circuit must be metallic, the material copper, and the product of the resistance of the line and its capacity must not exceed 7500 for very good, 5000 for excellent, and 2500 for perfect speech. A circuit approaching as nearly as possible one between London and Paris was made on an artificial cable, and found to comply with the requirements.

On the purification of sewage and water contaminated with organic matter by electrolysis, by Mr. W. Webster. The paper was divided into four sections, of which the fourth referred to the use of the electric current. The fact that water and the salts contained therein are easily decomposed if the current is of sufficient intensity is the explanation of the whole system. The changes taking place in sewage when electrolyzed depend chiefly on the splitting up into their constituent parts of sodium, magnesium, and other chlorides, nascent chlorine and oxygen being set free at the positive and the bases at the negative pole.

The strength of alloys at different temperatures by Prof. W. C. Unwin, F.R.S. In 1877, the Admiralty made some experiments as to the effect of variation of temperature on the tenacity of copper, Muntz metal, and phosphor bronze, and found that up to 500° F. the tenacity diminished proportionately with increase of temperature; in the case of gun metal, on the other hand, the tenacity diminished regularly up to 300° to 350°, but beyond this temperature there was a sudden reduction in the strength, which was found to be as low as half that at ordinary atmospheric temperature, whilst at 500° it became *nil:* the gun metal tested consisted of alloys of copper, tin, and zinc combined in different proportions. The author determined to make a series of experiments, the results of which he brought before the Association. The various alloys used, which are tabulated below, were heated in an oil bath, the temperatures employed, being all below that of the boiling of mercury, were read by means of a mercury thermometer. The results were plotted on a diagram, and given in tables, and show that the decrease of tenacity follows a regular law in each case; the temperature was given in degrees Fahrenheit, and the tenacity in tons on the square inch.

Temperature.	Yellow brass.	Cast brass.	Delta metal.	Muntz metal.	Gun metal.	Phosphor bronze.
Ordinary Fahr.	24·09	12·45	31·16	24·68	11·66	16·06
400°	21·23	11·11	26·58	20·84	11·06	12·30
500°	18·33	7·69	23·83	18·81	7·84	11·11
650° about ...	14·40	3·23	16·04	17·15 (615°)	4·82 (600°)	8·17

The influence of variation of temperature on the ductility of the same alloys was also experimented on, and was found to vary with the different alloys; with brass and gun metal there was little elongation before fracture; with Muntz metal it was considerable. In these cases the ultimate elongation diminished with increase of temperature, but in the case of Delta metal it increased. These experiments are of a very important character, and were carried out by the author on account of the very high pressures, and therefore temperatures, at which modern steam-engines are worked.

Central station heating and power supply, by Mr. W. W. Phipson. The system consists in the constant circulation of water at high temperature and pressure (viz. 400° F. and 250 pounds per square inch) from boilers at the supply station, through supply mains covered with non-conducting material, and back to the boilers by means of return mains, the circulation being maintained by pumps. Service boxes to supply the houses are fixed under the footpaths, which are connected to the mains by an inch pipe. From these boxes the house supply is taken by means of copper pipes. A vessel, called a converter, is fixed inside the house, whose use is to permit the water to resolve itself into steam, the pressure of which is controlled by means of a reducing-valve fixed on the copper pipe, before it enters the converter. From this converter the house services are taken. If a supply of both heat and power is required, double or compound converters are used with two reducing-valves, the power being taken from one and the heat from the other.

A curve ranger, by Mr. Alex. P. Trotter. The instrument is an application of the twenty-first proposition of the third book of Euclid, viz. the angles in the same segment of a circle are equal. A half-silvered mirror, such as is used in sextants, is mounted on an axis at one end of a bar, the other being provided with a sight. The motion of the mirror on its axis allows its inclination to the sight to be adjusted. To set out a curve, a pole is set up at each extremity, and the mirror is suitably adjusted. When the poles are seen, the one direct through the unsilvered part and the other by reflection in the silvered part of the mirror in apparent coincidence, and in the middle of the field as shown by the vertical line engraved on the mirror, the instrument is then at the point on the curve required. The mirror being clamped in position, the observer walks in the direction of the curve, and at suitable intervals places himself so that the poles at the extremities of the curve are seen in apparent coincidence.

On the application of the transporting power of water to the deepening and improvement of rivers, by Mr. W. H. Wheeler, M.Inst.C.E. The object of the paper is to show that the transporting power of water may be applied to the proposed purpose, and that under favourable conditions this can be accomplished by breaking up shoals, or the natural bed of a river, by mechanical agency and by mixing the material with the water, and allowing it to be carried away to the sea or estuary in suspension. The author has designed an improved apparatus, which, whilst disintegrating the shoal, mixes its material with the water, allowing it to be effectively transported by the ebb current clear of the channel to be improved.

THE ANTHROPOLOGICAL PAPERS AT THE BRITISH ASSOCIATION.

THE work of the Section commenced on Thursday, September 12, by Mr. Francis Galton, F.R.S., reading a paper on the advisability of assigning marks for bodily efficiency in the examinations of candidates for the public services. In the recent report of H.M. Civil Service Commissioners, they state that, a scheme of competition for physical qualifications having been brought before the notice of the War Office, it was not accepted, on the ground that the authorities were "completely satisfied with the physique of the young men who came to them through our examinations." The marks, as at present, of the candidates whose places lie near the dividing line between success and failure, run pretty evenly; therefore it is contended that the State would obtain better servants if such moderate marks were allowed for physical qualifications as to insure the selection of the most efficient in body from among those who are nearly on a par intellectually.

Mr. Galton also read a paper on the principle and methods of assigning marks for bodily efficiency. Two separate considerations are involved in the just determination of a scale of marks, which are usually mixed up in unknown proportions. (1) Absolute performance—on the principle that if the daily output of one man is greater than that of another, he should be more highly paid, or marked, in that proportion. (2) Relative rank—on the principle that superiority, however small, insures success

in competitions, and therefore the order of merit deserves recognition independently of the absolute amount of performance. The general conclusion is, that before proceeding to decide on scales of marks numerous measures should be discussed, made of persons of the same age and social class as the candidates, so that the quality of the men hereafter to be dealt with shall be statistically determined. The next step is to decide upon the relative weights to be allowed for absolute performance and for relative rank. Then, after a few other obvious preliminaries have been settled arbitrarily, consistent scales of marks could be at once drawn up.

A paper on left-leggedness, by Mr. W. K. Sibley, was read, in which the author contended that man is either naturally or artificially right-handed and left-legged, and, in walking, tends unconsciously to bear to the right; while the lower animals, on the other hand, appear nearly always to circle to the left. The left foot is more frequently the larger in the male than in the female sex, and the percentage of feet of the same size is greater in the female. The percentage of the right larger than the left is very constant, whereas the numbers of the left larger and those in which both feet are the same size are much more variable.

Prof. D. J. Cunningham read a paper on the occasional eighth true rib in man, and its possible relationship to right-handedness. In seventy subjects examined the anomaly occurred fourteen times, *i.e.* in 20 per cent. It was found twice in the male for every once in the female. Five cases were bilateral, nine cases were unilateral, and of these no less than eight exhibited the anomaly on the right side. From this Prof. Cunningham considered that it was just possible that the anomaly might have some connection with right-handedness.

The following papers were also read:—Dr. W. Wilberforce Smith, on the early failure of pairs of grinding teeth; Dr. Ridolfo Livi, on the development of the wisdom teeth; Prof. D. J. Cunningham, on the proportion of bone and cartilage in the lumbar section of the vertebral column in the apes and in different races of men. Prof. Cunningham also exhibited the model of the head of a man stated to be 106 years old, with the brain exposed, *in situ;* and the model of the head and shoulders of a young orang-utan, with the brain exposed, *in situ*.

On Friday, September 13, His Excellency Governor Moloney, C.M.G., read a paper on African airs and musical instruments. He distributed the airs geographically as follows:—A, Gambia; B, Ewe or Dahomey; C, Yoruba; and D, Houssa. In the first division specimens were given of *Bambara, Mandingo*, and *Volof* melodies, while *Popo* and *Dahomey* airs illustrated section B. The *Yoruba* division included *Lagos, Ibadan*, and other airs, and reference was made to several *Houssa* melodies. These countries were topographically described, and brief reference was made to their musical instruments and to the native minstrels. The paper concluded with an explanation of what is known as the "*drum language*."

Mr. Paul B. Du Chaillu read the next paper—the Vikings the direct ancestors of the English-speaking nations. The author described the early civilization and antiquities of the North-men, and dwelt upon the beauty of their ornaments and weapons, and also upon the similarity of Scandinavian and English ornaments belonging to the Early Iron Age, and the love of the Northern people for the sea. He spoke of the three maritime tribes of the North, according to the Romans, and of the fleets of the Sueones in the time of Tacitus; of the expeditions of the so-called Saxons and Franks, and of the home of these tribes; of the proofs from antiquities found in the North of the commerce of the North-men with the Roman Empire and with Greece; and also pointed out that the tribes of Germania were not a seafaring people, and were uncivilized, according to Roman writers. He gave an account of the probable origin of the names "Saxon" and "Frank," and spoke of the early settlements in Britain by the North-men during the Roman occupation, and of how the name of England might have been given to part of Britain. He alluded to the different countries of the Jutes, and how the language of the North and that of England was similar in early times, and that England was always called by the North-men one of the Northern lands. He mentioned the English and Frankish chronicles, in which the Sueones, Danes, and North-men are described, and that neither Saxons or Franks were a sea-faring people either at the time of Charlemagne or at any earlier period, and he dwelt on the mythical settlement of Britain by Hengist and Horsa, given by the English chronicles, which is quite contrary to the Roman records, Sagas, and archæology, and concluded by expressing his belief that the North-men, or Vikings, were the direct ancestors of the English people.

Canon Isaac Taylor contributed further researches as to the origin of the Aryans. Since the Manchester meeting of the Association in 1887, when the author read a paper on the same subject, he had re-examined the whole question from the anthropological rather than from the philological point of view. Assuming that there had been no migration of any new race into Europe since the Neolithic period, he contended that anthropologists have established the existence in Europe of four distinct prehistoric races, which might be reasonably connected with four existing types, which occupied nearly the same regions as the four prehistoric races. We have (1) the tall northern dolichocephalic race, the Canstadt race of De Quatrefages, which is the Scandinavian race of Penka, and the Eguisheim race of other writers. The stature of this race amounted to 5 feet 10 inches. It was platycnemic, prognathous, and dolichocephalic, with a mean cephalic index of from 72 to 73. The only pure descendants of this race are the North Germans and the Swedes. This Scandinavian or North-German type is maintained by Penka and other German writers to represent the primitive Aryans, who conquered the other European races and imposed on them their own Aryan speech. (2) We have a second type, also dolichocephalic, called the Silurian type by Prof. Rolleston, which is found in the long barrows of England. The normal stature was short, averaging 5 feet 4 inches; 6 inches less than that of the other dolichocephalic race. The cephalic index is between 73 and 74. This race was orthognathous, and swarthy, with dark curly hair, oval face, and feeble muscular development. It is now represented by the Welsh of Denbighshire, by the Irish of Kerry and Galway, by some of the Scotch clans, by the Spanish Basques, the Corsicans, the Sicilians, the Berbers, and the Guanches of the Canary Islands. (3) We have a tall northern brachycephalic race, represented in the round barrows of the Bronze Age in England, in the tumuli of Denmark and some caves of Belgium. The average stature was 5 feet 8½ inches, the mean cephalic index was 81. It was macrognathous—with projecting teeth and powerful jaws, a square powerful chin, and a face quadrangular rather than oval. It is almost certain that the hair was light, either red or reddish-yellow. It was, in all probability, the race which introduced Celtic speech into England, and is now represented by the tall, yellow, freckled Irish, by some Highlanders, by the Danes, and most of the Slaves, by the Esthonians, and by many Finno-Ugric tribes. (4) The fourth prehistoric race was also brachycephalic, but short in stature. It never penetrated to England, but is represented in the sepulchral caves of the Lesse in Belgium. The stature was from 5 feet to 5 feet 3 inches; the mean cephali index was 84; it was orthognathous and acrocephalic. It is now represented by the short dark population of Central France, more especially by the Auvergnats, the Savoyards, and the French Basques. It is found in the Rhætian Alps and among the Lapps. The hair is black and straight, and the eyes are dark. These four types and no others appear to have occupied Europe in the Neolithic period. It is difficult to find for them unexceptionable names, but we may for convenience call the first the Scandinavian type, the second the Silurian type, the third the Slavic type, the fourth the Auvergnat type. We have to determine which of these four races was probably the original Aryan race. The primitive Aryans must have either been by race Scandinavians or Slavo-Celts, and one must have imposed Aryan speech on the other. The Celts seem to have been in a higher stage of culture than the Germans, and therefore it is more probable that the Celtic race Aryanized the Teutonic race than that the Teutonic race Aryanized the Celtic race. Two hypotheses are possible—either the human race originated in Europe, bifurcating into the African and Asiatic races; or we may suppose the white or European race to have originated from the union of the yellow race of Asia and the black race of Africa.

Canon Taylor also read a paper on the ethnological significance of the beech. While the Latin *fagus* and the Gothic *boka* denote the beech, the word has come to mean the oak in Greek. The author endeavoured to show that the word *fagus* originally denoted the beech and not the oak, also that the Greeks entered Hellas from the north-west. The range of the beech is limited. It is a lover of chalk soils, and does not grow east of a line drawn from Königsberg to the Crimea. West of this line we must therefore put the cradle of

Latin, Greek, Celtic, and Teutonic peoples, as they had the same name for the tree prior to their linguistic separation. The Lithuanian and Slavonic tongues must have originated east of this line, as their name of the beech is a loan-word from the German. The early home of the beech seems to have been limited to France, Central and Southern Germany, Northern Greece, and Northern Italy. If, as has been contended, the cradle of the European Aryans was in Central Asia, where the beech is unknown, it is difficult to explain how the ancestors of Celts, Latins, Greeks, and Teutons migrating, at different times and by separate routes, to lands where the beech abounds, should have called it by the same name, modified in each case by the fundamental phonetic laws of the various languages. It is easier to believe that the cradle of the Aryans was, so to speak, astride of the beech line, the ancestors of Celts, Latins, Greeks, and Germans living to the west of it, and those of the Lithuanians and Slaves further to the east.

In a paper by Mr. Hyde Clarke, on the right of property in trees on another's land, as an origin of rights of property, the author stated that his attention was first called to the subject, as a Land-judge or Commissioner in Asia Minor, in 1862, in granting compensation for olive-trees belonging to one or more individuals on the fields of others, and for honey-trees or hoards of wild honey in State or Communal forests. The author had found evidence as to the existence of the custom in Borneo, with regard to Tapang or honey-trees, and in Chota Nagpore as to the Mhowa, a tree furnishing food, spirit, oil, &c. In China a lessee has the right to bamboo, &c., grown by him. The practice in the Turkish Empire he found extended into the European provinces, as applied to plum-trees in Bosnia. In Ireland it was recognized in the Brehon Laws as an individual property separate from tribal property. It is probable that the personal right of the first discoverer of honey and similar trees is to be regarded as the origin of an individual right of property rather than any right in land, which is of no value in a primitive community. Even cultivable land belonged to the community, and was distributed by lot yearly.

A paper by the Rev. J. Wilson, on an hypothesis of a European origin of early Egyptian art, was also read.

On Monday, September 16, Dr. Garson exhibited an anthropometric instrument specially designed for the use of travellers. This instrument occupies very little space, and its weight is scarcely more than that of a detective camera. It can be used for taking all measurements of length and diameter with ease and accuracy.

Mr. Francis Galton, F.R.S., exhibited an instrument for measuring the reaction time to sight and sound signals, and explained that they heard much about the quickness of hand and eye. When anyone saw or heard a thing he made a movement, and between the sight and the movement many physiological processes took place so quickly that the flash of lightning was nothing to it. The instrument he exhibited was intended to make an accurate measurement of the time which elapsed between the seeing or hearing of anything and the time occupied in making a certain movement afterwards.

Dr. Thomas Wilson gave an account of the Smithsonian Institution in the United States of America, and its work relating to anthropology.

In a paper by Dr. MacLaurin, on the British race in Australia, the author said that he did not think there was any distinct type of configuration in the Australian-born inhabitant which was sufficient to distinguish him from the ordinary Englishman, Scotchman, or Irishman. The muscular vigour of the British Australian race could be estimated by the readiness with which it entered into athletic exercises, and the result of this had been evident in the number of sculling champions and cricket teams that had recently visited this country. The population was increasing through the excess of births over deaths, which showed that the vitality of the race had not been diminished by transplantation to Australia.

Mr. H. H. Risley next read a paper on the study of ethnology in India. It was shown that the population of Northern India comprised three distinct types, viz. :—(1) A leptorhine dolichocephalic type of tall stature, fair complexion, and high facial angle, apparently corresponding in all points, except hair and complexion, with the Aryan type as defined by Herr Karl Penka, of Vienna. (2) A platyrhine dolichocephalic type of low stature, black or very dark complexion, and low facial angle. The wider racial affinities of this type are uncertain, and it is tentatively and conjecturally described as Australioid. (3) A mesorhine, platyopic, brachycephalic type of low stature, yellowish complexion, and low facial angle, described, in virtue of its low naso-malar index, as Mongoloid. The types thus worked out by anthropometric methods were shown to correspond with certain ethnographic groupings independently ascertained. In the Aryan and Australioid types the social status of each caste or tribe is found to vary inversely as its nasal index ; tribes with the highest index having the lowest social rank, and *vice versâ*. In the brachycephalic group social status appears to vary with the cephalic index. An attempt was made to deduce a theory of the probable origin of caste, and also to account for the custom of exogamy by the operation of the law of natural selection.

Prof. A. C. Haddon read a paper on some former customs and beliefs of the Torres Straits islanders. The natives of Torres Straits are divided into two distinct tribes—the eastern tribe, which inhabits Uga, Erub, and the Murray Islands ; and the western tribe, which occupies all the remaining islands. The islanders were divided into clans, each clan having some animal for its totem, such as the dugong, turtle, dog, cassowary, snake, shark, &c. The women used to have a representation of their totem cut on the small of the back. In the western tribe the lads on entering into manhood underwent a month's isolation in the bush. In the eastern tribe two elaborate ceremonies attended the initiation of the lads, but the discipline does not appear to have been so severe as in the other tribe. It was the custom in the western tribe for the women to ask the men in marriage. On the other hand, in the eastern tribe the men proposed to the women, and the women had to undergo a period of partial seclusion previous to marriage. The eating of food together was a feature in marriage. Belief in sorcery was universal, and all sickness and death were attributed to the charms of the medicine-man. There were also rain and wind makers.

Some observations on the natural colour of the skin in certain Oriental races, by Dr. J. Beddoe, F.R.S., was read. The author made numerous observations of this kind in the course of a voyage round the world. In most cases he found the colour of the clothed and protected body much lighter than is generally supposed. The capacity to tan, or become darker by exposure, varies much : thus the Melanesians are naturally lighter than the Australians, but they burn much blacker.

The following papers were contributed by Dr. R. W. Felkin : the normal temperature of the Soudanese, Negroes, and Europeans in Tropical Africa ; and the differences of sensibility between Europeans and Negroes, and the effect of education in increasing the sensibility of Negroes. Some anthropological notes collected by Mr. Edward Beardmore at Mowat, Daudai, New Guinea, were also read.

On Tuesday, September 17, Dr. Fridtjof Nansen read a paper on the Esquimaux. He said they were a race by themselves, and he did not think anthropologists agreed yet as to their real origin. He thought that tradition showed the Esquimaux really came from America. The Esquimaux of Greenland were now divided into two classes—those on the west coast who had been civilized by the Danes, and those on the east coast who were uncivilized. Esquimaux were seen in the north in 1823. The eastern Esquimaux had warmer clothes than the western ones. The young girls wore their hair loose, but afterwards they put it up in a knot at the top of the head, as a sign that they were ready for marriage. The Esquimaux, as a rule, lived in small tribes, and as many as ten families often lived in one hut. The Esquimaux had no written laws, but they had unwritten laws, which were kept strictly. The head of the house and the chief of the tribe was the best catcher of seals. As to property, they did not really know that word. No man had anything for himself, and any seals caught were divided between the families. They did not steal from each other, but they liked to steal from Europeans. Murder was not uncommon amongst the Esquimaux, and the punishment was really nothing at all. The men married as soon as they could catch sufficient seals to provide for a wife. Near relations, such as cousins, never married. On the east coast some men had two wives, the reason being that one wife could not prepare all the seals they caught. The children were not punished. Weak and deformed children, and those who lost their mothers, were as a rule thrown outside the house or into the sea. Old people, who were ill, were often thrown into the sea. He thought the time would come when the Esquimaux would be extinct.

The Rev. G. Rome Hall read a paper on Northumberland in prehistoric times. On the east coast of England no trace of the Cave-men had as yet been found further north than Norfolk.

We came, after an immense and unknown lapse of time, to the Neolithic period, when the earliest inhabitants of Northumberland, who were, so far as can be ascertained, cognate with the Basques and Lapps, crossed the Tyne in small family or tribal bands. Though probably never numerous, their polished weapons and implements had been frequently found. Considerable hoards of bronze had been found near Alnwick, Rothbury, and Wallington. Beads of gold were discovered in a barrow at Four Laws, or Chesterhope. Near Bellingham, in North Tynedale, a gold armlet was found. Burial by inhumation was customary in the later Stone Age, and cremation followed. Interments were sometimes in split oak coffins, found at Featherstone, but usually in stone-lined graves, the body being doubled up as in the posture of sleep, sometimes with an iron food-vessel placed near the head. The author explained the migrations of the people in early times. To the Iron period we owed the introduction of the greater part of the names of local mountains, hills, rivers, and streams, as the Tyne from "don" or "tan," the water. The bronze-using invaders may have landed in England about B.C. 1000, and the Iron Age in Northumberland might have begun about B.C. 500 or 400. Modern Northumbrians, he concluded by remarking, might perchance owe more than they thought to the combination of racial characteristics resulting from the continuity of life proceeding from even prehistoric times down to the present day.

Sir William Turner read a paper on implements of stags' horn associated with whales' skeletons found in the Carse lands of Stirling. He showed that skeletons of whales had been found, together with implements of stags' horn. The discovery of these horn implements showed that when the fertile land now forming the Carse of Stirling was submerged below the sea-level, the surrounding high lands were inhabited by a hardy Caledonian race, who manufactured useful tools and weapons from the antlers of the red deer. It was probable that the whales had been stranded during the ebb of the tide, and that the people had descended from the adjacent heights, and, with the aid of their chisels of horn, had spoiled the carcass of its load of flesh and blubber. There was nothing in the shape of those implements to lead anyone to suppose that they could be used in the chase of the whale. The period of this people was probably covered by that termed the Neolithic, the termination of which was stated to be from 5000 to 7000 years ago.

Mr. Bernard Hollander read a communication on the relations between brain-functions and human character, with the view of showing the possibility of a scientific phrenology.

The following papers were also read:—Prof. G. J. Romanes, F.R.S., on the origin of human faculty; Prof. Frazer, on a new method of illustrating the topographical anatomy of the brain; Mr. George Weddell, notes on classification in sociology; Mr. S. B. J. Skertchly, on fire-making in North Borneo, and on some Borneo traps; Mr. James Macdonald, on manners, customs, and superstitions of South African tribes.

THE MAORIS.

AN unusually lengthy Report from the Registrar-General of New Zealand on the condition of that colony, which has lately been laid before Parliament, contains some interesting information respecting the present condition of the Maoris. Mr. Brown says that, according to the traditions of the Maoris, their ancestors first arrived in New Zealand from an island in the Pacific Ocean, to which the name of Hawaiki is given. Since that event it appears, from genealogical sticks kept by the *tohungas*, or priests, that about 20 generations of the race have lived. The number of the Maori race at the time of the first foundation of the colony, in 1840, was estimated at about 80,000. Twenty years previously the number had been estimated at 100,000. In 1857 an enumeration of the race was made, from which it appeared that the number of males was then about 31,667, and that of the females about 24,303; and of those whose sex was not stated, 79; a total of 56,049. Subsequent attempts at enumeration have been made; but, owing to the objections felt by natives to stating their numbers, and to the difficulties experienced in obtaining information in those parts to which the European was not allowed free access, with not wholly satisfactory results. The latest, and probably most accurate, of these enumerations, was made in 1886. This gave the number of males as 22,840, and the females as 19,129; a total of 41,969.

That there has been a serious decrease in the numbers of the race of late years is the general opinion of all competent to judge, and a consideration of the numbers of each sex, and the proportion living at each age-period, leads to the conclusion that in all probability the decrease is still progressive. In 1886 the proportion of females was 83·75 to every 100 males. In the European portion of the population the proportion was 85·28 females to 100 males. The proportions are not relatively comparable, as the excess of European males over females is caused by immigration; but there is no external cause to account for the Maori males being more numerous than the females. In the European portion of the population, under 20 years of age, the proportion was 100 males to 99 females; in the Maori population under 20, the proportion was 100 males to 87 females. The males under 15 years of age were in the proportion of 31·82 to every 100 of the male population, and the similar proportion among the females was 33·59; these being less than the proportions in 1881—an evidence of a low birth-rate, or high juvenile mortality, leading to a racial decrease.

On comparing the proportions living at each quinquennium under 20, and each decennium above that period, with the corresponding proportions in the population of England, and that of the New Zealand European, it is found that at all ages under 20 the proportions among the Maoris are far less than among the other two populations, and at each age-period above 40 the Maori proportions are far higher. It is, of course, a fair inference that the causes of these larger proportions at the higher age-periods are two-fold—namely, a low birth-rate, and a high death-rate among the younger members of the community. This is borne out by the much smaller proportions of young children to those in either the English or New Zealand European communities. The smaller proportion of females (83·75) to males (100) also shows a greater mortality among the adult females than among the males, as 42·29 per cent. of the females living were under 20 years of age, but only 39·70 per cent. of the males were under 20. The manifest decrease in the numbers of the race is much to be regretted, for the Maoris show great aptitude for civilization, and they possess fine characteristics, both mental and physical, and rapidly adopt the manners and customs of their civilized neighbours. In mental qualifications they can hardly be deemed as naturally an inferior race, and the native members of both the Legislative Council and the House of Representatives take a dignified, active, and intelligent part in the debates, especially in those having any reference to Maori interests. The Maoris contribute largely to the taxation of the country through the Customs duties; and, having regard to the relations now subsisting between the races, they may be regarded as constituting an important element of strength in the population of the colony.

On the subject of the education of the natives, Mr. Brown says the number of native village schools at the end of 1887, either supported or subsidized by the Government, was seventy-nine, an increase of eight on the number in 1886. In addition, there were two more subsidized private schools for the education of Maori children only, and seven boarding-schools for native children, the cost of whose maintenance was paid either by Government or out of endowments. The number of Maori children attending school at the end of 1887 was 2812, viz. 1612 males and 1200 females. These included children of mixed European and Maori blood, who live as members of native tribes. The following is a statement of the number of Maori children who were attending schools in 1886 and 1887:—

	1886.	1887.
At public European schools	475	343
At native village schools	1910	2215
At subsidized or endowed boarding-schools	162	156
At private European or native schools	100	98

There was thus an increase of 165 on the number of native children who were being educated in 1887. There was a decrease of 132 in the number attending European public schools, but an increase of 305 on the number attending the native village schools.

The information supplied respecting the age of the Maoris at the census of 1881 was very incomplete, and therefore only a merely approximate estimate can be given as to the numbers living at the usual school ages, 5 to 15. Out of a Maori population of 22,840 males and 19,129 females, the ages of 21,724 males and 17,936 females were given as either under or over 15 years. The proportion of those under 15, if applied to the whole of the population, would give 7226 males, and 6420

females under 15 years of age. If it be assumed that the numbers living under 5 years of age bear the same proportion to the whole number under 15 years of age as in the European portion of the population, the above numbers would give 4596 Maori males and 4082 Maori females between 5 and 15 years of age. As all the children attending the private and public schools may be fairly taken at over 5 and under 15 years of age, and the ages of those attending native schools are ascertainable, it may be roughly stated that nearly 32 per cent. of the Maori boys and nearly 27 per cent. of the Maori girls between 5 and 15 years of age attend schools.

SCIENTIFIC SERIALS.

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. xxii. fasc. xv.-xvi.—It appears from experiments here described by Signor Sormani that the Bacillus and spores of tetanus may be drawn into the respiratory passages by inhalation, or even injected into the bronchial tubes, without producing tetanus. The Bacillus is probably anaërobic, unable to develop in presence of oxygen. The tetanus called *rheumatic* is thought to be of traumatic origin really, the wound being slight, and but little of the virus introduced. Tetanus is most common in Northern Italy; its maximum being in Lombardy and Emilia, where people frequently work, in the hot season, with bare feet. They are attacked in the proportion of 100 males to 30 females. The mortality in hospitals is about 44 per cent of those attacked.—We note two medical papers: on recent innovations in treatment of free inguinal hernia (Signor Scorenzio), and on fibroma of the breast (Signor Sangalli).—An inquiry into the nature and uses of the *stufe* and warm baths of the middle and later ages is summarized by the author, Signor Corradi.—Signor Maggi writes on the principles of the theory of potential functions, and there are continuations of mathematical papers by Signors Oschieri and Giulio.

Bulletin de l'Académie Royale des Sciences de Belgique, No. 8, 1889.—M. Massart here seeks to account for the penetration of spermatozoïds into the egg of the frog by the resultant attraction of the gelatinous mass round the egg, which, absorbing water, presents a gradually increasing density inwards. If a piece of the episperm of linseed or quince be put in water containing spermatozoïds, the mucilaginous matter round it swells similarly, and, during the twenty minutes this continues, the spermatozoïds are attracted, and make their way to the centre. When the absorption ceases, they stop too. The gelatinous covering of the frog's egg, separated from the latter, in water, affects them similarly. M. Massart holds that sensibility to contact is the explanation of the phenomena (not mechanical attraction, nor a sense of the direction of the current).—M. de Heen describes a simple new apparatus for measuring the heat conductivity of some homologous organic liquids, and shows that, in a given series, the conductivity diminishes with increasing molecular weight; but the square of $\frac{1}{c}$ varies generally less rapidly than the weight. He also discusses the dilatability of liquids in relation to molecular movements.—M. Henry studies the volatility of normal cyanic ethers, and of poly-oxygenated carbon compounds; finding the simultaneous presence of oxygen and nitrogen, or the accumulation of oxygen, at one point in the molecules, a powerful cause of it. He has also a short paper on monohaloid ethers of ethylenic glycol.—M. Dewalque supplies some phenological figures for Liége, Spa, &c. A valuable paper on the svastika appears in the section *des lettres*.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, October 2.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—Mr. F. P. Pascoe exhibited a number of species of Coleoptera, Lepidoptera, Hymenoptera, Neuroptera, Hemiptera, Orthoptera, and Diptera, collected by himself during the past summer at Brindisi, and in Greece and the Ionian Islands.—Mr. J. W. Douglas sent for exhibition specimens of *Lygus viscicola*, Puton, a species new to Britain, taken at Hereford, in September last, by Dr. T. A. Chapman.—Mr. R. McLachlan, F.R.S., exhibited nearly 100 specimens of Trichoptera recently collected in Iceland by Dr. P. B. Mason. Only six species were represented, and of these five had been previously recorded from the island. Mr. McLachlan remarked on the great amount of variation existing in some of the specimens.—Mr. E. B. Poulton, F.R.S., exhibited a mounted specimen of the yellow powder from the cocoon of *Clisiocampa neustria* under a power magnifying 188 diameters. The powder was thus seen to consist of crystals so minute that the form could only just be made out under this power; it was present in a crystalline form in the Malpighian tubules, and discharged from the anus of the larva. A discussion ensued as to the functions of the Malpighian tubes, in which Mr. Stainton, F.R.S., Lord Walsingham, Dr. P. B. Mason, Mr. McLachlan, and Dr. Sharp took part.—Mr. Poulton also exhibited some photographs of the living larvæ of *Hemerophila abruptaria*, showing different depths of colour which had been induced by experiment; the larvæ had been rendered very light in colour by being surrounded by green leaves and stems only, whereas they had become extremely dark when numbers of dark twigs were intermingled with the leaves of the food-plant. Mr. F. Merrifield said that Dr. Chapman had recently obtained similar results from experiments made with the larvæ of *Ennomos alniaria*.—The Rev. Dr. Walker exhibited, and read notes on, a number of Coleoptera, Neuroptera, Hymenoptera, and Diptera, which formed the second instalment of the collection which he had recently made in Iceland.—Mr. R. South exhibited a specimen of *Luperina nickerlii*, Freyer, caught in Lancashire last August. He also exhibited, and read notes on, a long series of *Boarmia repandata*, bred from larvæ collected in North Devon. Mr. Poulton, Mr. Merrifield, and Lord Walsingham took part in the discussion which ensued.—Mr. J. J. Walker, R.N., exhibited a collection of Coleoptera made during the past summer in Cobham Park, Kent. Thirty-three species were represented, amongst which were the following, viz. *Eros minutus*, *Philonthus fuscus*, *Homalota hepatica*, *Abræus granulum*, *Anisotoma grandis*, *Agaricophagus cephalotes*, *Thalycra sericea*, *Cryptophagus ruficornis*, *Platytarsus setulosus*, &c.—Herr Jacoby exhibited a curious Phytophagous beetle found by Mr. J. H. Leech in the Corea. He stated that he was unable to determine the species, as was also Mr. J. S. Baly, to whom he had submitted the specimen.—Mr. R. Adkin exhibited specimens of *Retina resinella*, received by him from Forres. Lord Walsingham remarked that he had never seen the species in Scotland, but that it was not uncommon in Germany.—Mr. W. Dannatt exhibited a male specimen of *Papilio antimachus*, Drury, received from Lukolela, a station about 500 miles from the mouth of the Congo. He stated that the species, although very rare, had a wide range, as three other specimens of it had been received from the Stanley Falls, which were more than 800 miles further up the Congo.—Lord Walsingham exhibited specimens of the larva and imago of *Cidaria reticulata*, collected in the Lake District, and sent to him by Mr. Hodgkinson.—Mr. J. Jenner-Weir exhibited fore-wings of the males of *Argynnis paphia*, *A. adippe*, and *A. atlantis*, denuded of the scales, in order to show that there was no dilatation or thickening of the median nervules and submedian nervure in that sex of these species; but that the apparent dilatation was produced by a dense mass of scales crowded together on each side of the nervules. He also read a short paper on the subject entitled "Notes on the Nervules of the Fore-Wings in the Males of *Argynnis paphia* and other Species of the Genus." Mr. Jenner-Weir said he was supported in his views by the opinions of Mr. S. H. Scudder, Dr. Staudinger, and Dr. Schatz.

SYDNEY.

Linnean Society of New South Wales, July 31.—The following papers were read:—Description of a new species of Iodis, with remarks on *Pielus imperialis*, Olliff, by Thomas P. Lucas. For the new species of Iodis—of which three specimens were recently captured in Brisbane by Mr. Illidge—the name of *P. illidgei* is proposed. The second part of the paper consists of critical remarks on *P. imperialis*, Olliff, which the author states is identical with *P. hyalinatus*, Schäffer.—The examination of kinos as an aid in the diagnosis of Eucalypts; Part 1, the Ruby Group, by J. H. Maiden. The author refers to a previous paper, in which he shows that Eucalyptus kinos may readily be grouped into three great classes, according to their behaviour with water and with spirit. Briefly, he divides them into (1) the Ruby Group, which consists of ruby-coloured kinos, the members of which are soluble either in cold water or in cold spirit. (2) The Gummy Group, whose members are soluble in cold water, but very imperfectly in spirit, owing to the gum they contain. (3) The Turbid Group, whose members are soluble in hot water or in hot alcohol, but the solutions become turbid on cooling, owing

to the presence of catechin. He then deals with the first group, and shows that, with one anthereally doubtful species, the members are identical with the group Renantheræ of Bentham and Müller's anthereal classification. He shows how the examination of kinos is a valuable aid or supplement in the diagnosis of Eucalypts, and concludes this part with an account of all the ruby kinos at present known to science.—On Rhopalocera from Mount Kosciusko, New South Wales, by A. Sidney Olliff. In this short paper some sixteen species are recorded from specimens obtained by Mr. R. Helms, a most painstaking and energetic collector, who recently made an excursion, chiefly in the interests of entomology, on behalf of the Australian Museum. The collection contains both the species described from the mountain by Mr. Meyrick, as well as a new Xenica, proposed to be called *X. correæ*.—Note on the fructification of *Phlebopteris alethopteroides*, Etheridge, fil., from the Lower Mesozoic Beds of Queensland, by R. Etheridge, Jun. From the examination of additional material the author has been able to determine an arrangement of the sori similar to that in *P. polypodioides*, Brongn., and other known species of the genus.—Note on the bibliography of Lord Howe Island, by R. Etheridge, Jun. This paper is supplementary to a recently-published work ("Lord Howe Island: its Zoology, Geology, &c.," *Mem. Austr. Mus.*, 1889, No. 2), and gives a digest of certain valuable reports by Dr. Foulis, Mr. White, Captain Denham, R.N., and Dr. J. Dennis MacDonald, contained in the "Votes and Proceedings of the Legislative Council of New South Wales for 1853," and with which, when contributing to the above-mentioned work, the author had been unable to meet

PARIS.

Academy of Sciences, October 14.—M. Des Cloizeaux, President, in the chair.—Presentation of vol. iv. of the "Collection of Memoirs relating to Physics," published by the French Physical Society, by M. C. Wolf. This volume is devoted to the pendulum; and contains memoirs by La Condamine, Borda, Cassini, Prony, Kater, and Bessel. M. Wolf supplies a bibliography and chronology of works on the pendulum from Galileo's time to 1885; also an historical introduction. The fifth volume will deal with the same subject.—Reciprocal displacements between the halogen elements and oxygen; hydrobromic and hydriodic acids, by M. Berthelot. A *dilute* solution of iodide of potassium remains an indefinite time colourless in presence of oxygen; but it is otherwise with a *saturated* solution, owing to the formation of a small amount of tri-iodide. Dilution of the yellow liquor with fifty times its volume of water (or more) removes the colour almost entirely; dissociation of the tri-iodide allowing the potash to react fully with the iodine.—On transformism in pathogenic microbiology; limits, conditions, and consequences, of the variability of the *Bacillus anthracis*; researches on ascendant or reconstituant variability, by M. A. Chauveau. The natural Bacillus, with its virulence quite removed by compressed oxygen, may be revivified by degrees, thus: it is cultivated in *bouillon*, to which fresh blood of, *e.g.*, a guinea-pig is added, and in very rarefied air; it then becomes fatal to mice, guinea-pigs, rabbits, &c., and is vaccinal to small ruminants, but does not kill them. Cultivation of this Bacillus in *bouillon* to which sheep's blood is added renders it fatal to small ruminants, and probably vaccinal to the ox.—New relation between sugars and furfuric compounds; constitution of methylfurfurol and of isodulcite, by M. Maquenne. Distilling isodulcite ($C_6H_{12}O_5$) with dilute sulphuric acid, he got some pure methylfurfurol ($C_6H_6O_2$) identical with that obtained from Fucus; and he infers the presence of isodulcite in tissues of marine plants. Its relations to arabinose suggest that it may be much more widely diffused than has been supposed.—On the physical properties of the free superficial layer of a liquid, and of the layer of contact of a liquid and a solid, by M. Van der Mensbrugghe.—On doubly harmonic linear elements, by M. L. Raffy.—On the area of certain ellipsoidal zones, by M. G. Humbert.—On the fermentation of raffinose, in presence of different species of beer-yeast, by M. D. Loiseau. A claim of priority.—Observations on the communication made by M. Ch. E. Guignet, at the meeting of September 30 last, by MM. C. Vincent and Delachanal. The addition of ammoniacal sulphate of copper to the juice of sorbs precipitates sorbite itself, so the production of this precipitate does not prove the presence of mannite nor its separation from sorbite.—On the optical analysis of oils and of butter, by MM. E. H. Amagat and Ferdinand Jean. They describe a method based on variation of the index of refraction of various oils, and of the melted fatty matter of butter, due to the presence of adulterating substances.—On air contained in the soil, by M. Th. Schlœsing, *fils*. He has improved on the method adopted by MM. Boussingault and Léwy thirty years ago; he forces into the ground a steel tube with conical point, the opening of which is temporarily closed by wire. The upper end is connected by means of a capillary tube with a bulb, from which mercury is withdrawn on lowering a small connected reservoir; thus the air of the soil is drawn in. He finds abundant gaseous oxygen in the soil, and much variability at different times; details are promised.—On a musculo-cutaneous strip, in form of a flap, applied to the restoration of eyelids, by M. Léon Tripier. The strip is dissected out from one eyelid and transferred to the other side.—On the exploration and the formation of *avens*, by MM. E. A. Martel and G. Gaupillat. These *avens* are natural, open, deep pits, found in numbers on calcareous plateaus. The authors hold that four factors participate in their formation: (1) previous dislocations of the ground; (2) surface waters (erosion); (3) interior waters (erosion, hydrostatic pressure, falling in); (4) chemical phenomena. Frequently only three or two of these factors have been in operation. It is only accidentally that the *avens* communicate with subterranean rivers.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Chief Ancient Philosophies—Aristotelianism: Rev. I. G. Smith and Rev. W. Grundy (S.P.C.K.)—Toilers in the Sea; M. C. Cooke (S.P.C.K.).—Federal Government in Canada: J. G. Bourinot (Baltimore).—Elementary Manual of Magnetism and Electricity; Part 1, Magnetism: Prof. Jamieson (Griffin).—Index of the Genera and Species of Mollusca in the Hand-list of the Indian Museum, Calcutta, Parts 1 and 2 (Calcutta).—Journal of the Chemical Society, October (Gurney and Jackson).—Journal of Anatomy and Physiology, October (Williams and Norgate).—Morphologisches Jahrbuch, 15 Band, 2 Heft (Leipzig).—Journal of the Royal Microscopical Society, August (Williams and Norgate).—Key to Lock's Arithmetic for Beginners: Rev. R. G. Watson (Macmillan).—A General Formula for the Uniform Flow of Water in Rivers and other Channels: E. Ganguillet and W. R. Kutter; translated (Macmillan).—Scientific Papers of Asa Gray, 3 vols., selected by C. S. Sargent (Macmillan).—Hand-book of the Bromeliaceæ: J. G. Baker (Bell).—Ker Kompass an Bord; ein Handbuch für Führer von Eisernen Schiffen (Hamburg, Friederichsen).—A Bibliography of Geodesy; Appendix No. 16, Report for 1887 (Washington).—Calendar of the University College of Wales, Aberystwyth, 1889–90 (Manchester, Cornish).—Les Industries des Animaux: F. Houssay (Paris, J. B. Baillière).—Glasgow and West of Scotland Technical College Calendar for the Year 1889-90 (Glasgow, Anderson).—The Engineer's Sketch-book: F. W. Barber (Spon).—Proceedings and Transactions of the Royal Society of Canada for the Year 1888, vol. vi. (Montreal, Dawson).—Iris; Studies in Colour and Talks about Flowers: F. Delitzsch, translated by Rev. A. Cusin (T. and T. Clark, Edinburgh).—Steam: W. Ripper (Longmans).—The Tornadoes and Hailstorms of April and May 1888 in the Doab and Rohilkhand: S. A. Hill (Calcutta).—Journal of the Royal Statistical Society September (Stanford).—Journal of the Scottish Meteorological Society, 3rd series, No. 6 (Blackwood).—Journal of Morphology, vol. iii. No. 1 (Boston, Ginn).

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